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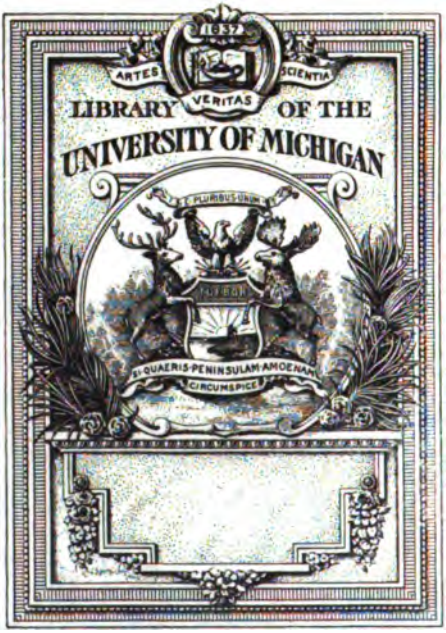

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EDITED BY

W. H. M. CHRISTIE.

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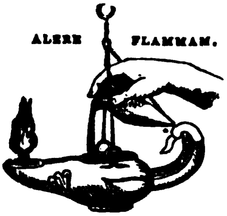
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THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 13. MAY 1. 1878.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 12th April, 1878.

Lord Lindsay, *President*, in the Chair.

Secretaries: Mr. J. W. L. Glaisher, F.R.S., and
Mr. A. Cowper Ranyard.

The Minutes of the last Meeting were read and confirmed.

Mr. Glaisher announced that 66 presents had been received by the Society since the last Meeting in February, none of them calling for special notice, excepting a number of volumes of Transactions which had been sent by the Vienna Academy to complete the Society's set, which had hitherto been very deficient. The thanks of the Society were voted to the donors.

Capt. Abney read a paper on Photography at the least refrangible end of the solar spectrum, and exhibited, by means of the oxy-hydrogen light, a number of beautiful photographs of the ultra-red end of the spectrum, in which numerous lines beyond A were shown exquisitely defined. The first photograph shown was obtained with a spectroscope having three prisms of dense flint, an orange-coloured glass being placed in front of the slit to cut off scattered light from the green and blue rays. In this photograph the spectrum with numerous fine lines could be traced to a wave-length of at least 10400 tenth-metres, or as far beyond A (7600 tenth-metres) as that line (the limit of the visible spectrum) is from F. But, on account of irrationality of dispersion, the red end is much compressed, and recourse was therefore had to a diffraction-grating. A photograph of the spectrum between C and A, taken with it, was exhibited for comparison with Prof. Piazzi Smyth's drawing of the same part; and then a plate of the ultra red, in which a remarkable group of lines, about 8400, and another group at 10,000, were shown. These were taken with exposures of 15 and 20 minutes respectively.

Capt. Abney described his process as a weighting of the molecules of silver bromide either with resins or, in his later experiments, with silver bromide itself, so as to form double molecules; but he could not easily explain it, as it was rather a matter of manipulation than of chemical constitution. In illustration of this a plate coated with the sensitive salt was shown, which appeared blue by transmitted light, but on being rubbed with the finger became of a strong red hue. A plate in this latter state, which had been exposed to the whole spectrum, was shown on the screen, and the red was seen to be as dense as the blue, whilst the yellow was altogether wanting. Another plate similarly exposed immediately after, but prepared in a different manner, showed a much more vigorous image in the yellow than in the red. Capt. Abney also referred to the oxidizing effect of the red rays as offering an explanation of the reversal of the spectrum of the red end, and showed a photograph which had first been slightly exposed, and then submitted to the spectrum in an oxidizing solution. In this case an ordinary image was obtained of the blue end and a reversed image of the red, the lines being transparent for the former and opaque for the latter. It was this oxidizing action which caused the difficulty of photographing the red rays, as no image is formed where it predominates. In conclusion, Capt. Abney trusted that this communication would be accepted as a redemption of the pledge implied in his preliminary note on the same subject.

The President. Before asking the Society to express their thanks to Capt. Abney for his most interesting paper, I would invite discussion from those present. I think Dr. De La Rue has given great attention to this subject.

Dr. De La Rue. I think Capt. Abney has done wonderfully good service in promoting the power of photography by extending its range to the ultra red. It will be remembered that M. Becquerel some years ago produced a photograph in colours. Unfortunately it would not bear exposure to the light. But the wonderful power which Capt. Abney has over the chemicals which he employs, and his knowledge of their physical constitution, lead me to ask whether he himself has worked in the direction of producing coloured photographs. I would also ask another question, and he will perhaps kindly answer both at the same time. Latterly, in America, Dr. Draper, Jun., the son of that most distinguished physicist who did so much to develop the powers of the Daguerreotype, has been working to obtain the spectrum of oxygen in the Sun; and I would like to know whether Capt. Abney has any thing that he can adduce in confirmation of the remarkable work of Dr. Draper.

Capt. Abney. In my paper read at the Royal Society I mentioned that I have every reason for believing that those coloured photographs are simply states of oxidization of the photographic image; and I think I can produce them at pleasure in the same way as I produced these photographs at pleasure. That is a matter which I do not care to enter into at present. With regard to the oxygen of the Sun, far be it from me to pass any criticism upon Prof. Draper, except upon one point, which is not connected with oxygen in the Sun; and that is, that last year Dr. Draper, Sen., kindly noticed my work as regards the spectrum, and said that it would be very much better if I would leave off trying to take proper negatives of the red end of the solar spectrum, and stick to his plan of getting reversed negatives; and he suggested that some people should pay more attention to this particular subject, as to the reasons that this reverse action took place. I have been for a long time working at the subject, and the result is that I think I have satisfactorily answered Prof. Draper's demand that something should be known of the reason for the reversing of the action on the red rays of the spectrum.

The President. The subject of these photographs of the red end of the spectrum is one of such great interest that I would ask Capt. Abney how he manages to focus for those invisible rays. I suppose it is a matter wholly tentative; but at the same time there was one negative thrown on the screen in which the red end and the violet end of the spectrum were also to be seen. I understood Capt. Abney to say that the blue end of the spectrum was cut off.

Capt. Abney. All the blue was cut off by the red glass, so that there was nothing but the red that you saw.

The President. I understood you to say that the yellow was cut off, and I wanted to ask how you managed to focus it all on your plan.

Capt. Abney. I may say it is not in focus. It was only a tentative experiment to see the relative sensibility of the blue and the red. I should like to call your attention to Prof. Piazzi Smyth's map, which I hope will be compared with the photographs, and I think you will see that there are even more lines than Prof. Smyth has shown. In the ultra red there are lines which I do not think anybody has seen at all. I did not see them until a short time ago; and I was astonished at the different beautiful groupings that presented themselves. When you come to wave-length 12000 the groupings are very remarkable indeed, so much so that I had hesitated to bring them forward to-night at all.

The President. When you got the locality of those peculiar lines, did you try any temperature experiments with them? did you find, by the thermopile, whether there was any aggregation of heat at such a great distance from the known action of heat?

Capt. Abney. I am undergoing what may be called a process of pumping. Thermopile experiments are part of my programme. The focussing of the ultra red is, I may say, a matter of the very greatest difficulty. Only last night I found, in my laboratory, that somebody had moved my condenser; and the consequence was that, when I tried to get a photograph of one particular line, I found that I was nowhere. The condenser had been shifted, and I could not get the photograph anywhere near the focus. It is merely a matter of trial and error. It has been calculated by the radius of the circle, but practically it is not true.

Mr. Ranyard. May I ask Capt. Abney two questions? In one of the photographs which he has shown to-night the lines are as sharp as if they had been drawn with pen and ink; may I ask whether that was an actual photograph of the spectrum, or a photograph of a drawing?

Capt. Abney. It was a genuine photograph of the spectrum. Some people said it was a photograph of a drawing; but I think anybody who examined the internal evidence would see that it is a true photograph of the spectrum of the ultra-red rays.

Mr. Ranyard. You said that they were taken with a diffraction-grating: were there any other spectra, or had you isolated a particular spectrum, which you photographed?

Capt. Abney. I tried to explain that in my paper. I separated one from the others by the old method of the prism, and the others were cut out entirely.

The President. I am sure that the Society will give their most cordial thanks to Capt. Abney for his most interesting communication. I may only add that when communications of such interest come before the Fellows, the Fellow communicating such things must not mind a little cross examination or pumping. No doubt to be pumped is not always pleasant; but it is very pleasant for those who get information by the action of the pump-handle.

Dr. De La Rue. Will the President permit me to make one remark?—that, fortunately for us, the well is not dry.

A vote of thanks to Capt. Abney for his interesting paper was carried unanimously.

The President. I have received a communication from M. Fizeau of Paris. At least, it was sent as a communication to the Scientific bodies of England in general, requesting that it might be made known to them that proceedings are being taken in France for the erection of a memorial to the late M. Le Verrier. It is in these terms:—

"*From the President of the Committee for the erection of a Statue to* M. LE VERRIER, *the late Astronomer of Paris, to the* ASTRONOMER ROYAL.

"I am desired to inform you that a Committee has been formed to honour the memory of M. Le Verrier, and to collect subscriptions in order to raise a monument to this eminent Astronomer. The Committee at present consists of the President, the Vice-President, the two permanent Secretaries, and a great number of the members of the Academy of Sciences. I have therefore the honour to ask your permission to place your name upon the list of members of this Committee; and I ask you if it will be possible for you to aid the work which we have undertaken in bringing before, and recommending it to all the Scientific world amongst whom you move.

"I have the honour to be, Sir, your obedient Servant,

"H. FIZEAU, *President of the Academy of Sciences, and President of the Committee.*

"Letters on this subject should be addressed to the Secretaries of the Institute of France, where a bureau for registering subscriptions is open."

It is as well that this paper should appear in the 'Monthly Notices,' and then any Fellow of the Society who likes to contribute his mite towards raising a statue to so great a man will have ample facilities for doing so.

The Astronomer Royal. I think it desirable that the Society should consider what has been done in other cases of a similar character, two or three of which have occurred in my time. One was the erection of a monument to Sir Isaac Newton at Grantham, on which occasion I am not aware whether any step was taken to solicit the contributions of foreign members of the Society. I speak under correction, but I certainly think that what was done then was done entirely by Englishmen. In a much later case, that of M. Gauss, circulars were addressed to many persons in England, but I do not think that much, if any thing, was done in this country. My own view, at the present moment, of the feeling to be entertained in matters of this kind, is a feeling of local pride in reference to the education and nutrition and ultimate welcome given to the men who have distinguished themselves in science in any country by the scientific persons of that country. There is one thing of which I am quite sure, that any person who knows what has passed in the last 20 or 30 years in the intercourse between the two nations can have no doubt as to the high appreciation that I entertain for that monster of science M. Le Verrier. I am speaking entirely in an abstract way with regard to the exclusive claim which it may be considered that each nation has to the celebration of its own men of science.

The President. The suggestion has been brought forward that the Society should subscribe to this; but I fancy that that would be impossible, as by our charter we should not be permitted to devote our funds to such an object, however worthy.

Mr. Ranyard read a paper from Mr. Ellery on observations of the polar and equatoreal diameters of Mars near its opposition in 1877.

Mr. Christie. I have received a letter from Mr. Ellery in which he mentions incidentally that he has made a number of drawings of Mars which he will be happy to send to the Society if they are likely to be of any use; but at the same time he said that he found the definition was generally very bad indeed; and, so far as I can gather, he seemed to think that the planet Mars was rather an unfavourable subject for telescopic study.

Mr. Jenkins read a paper on the luminous spot on Mercury in transit.

In the paper the author divided the transits of Mercury into May transits and November transits. He found that of those which had occurred within the last 170 years, there were thirteen upon which some observations had been made in regard to the luminous appendages; but, to come nearer to our own time, there were in all six within the last 45 years, of which four had occurred in November and two in May, and that a May transit had not occurred since 1845. He believed that this division of transits would throw considerable light on the subject, because Mercury was in two opposite points of its orbit in these two months—in November being only about five days from perihelion, and in May about ten days from aphelion.

After a careful consideration of the whole of the available evidence, the author drew the following conclusions :—

1. That in the May transits the luminous spot on the disk is in advance of the planet, preceding the centre; but in the November transits it follows the centre.

2. That the luminous spot has never been seen at the centre, but always a little south of it.

3. That sometimes in the same transit two spots have been seen close together, where shortly before only one was seen.

4. That in the May transits the rings round the planet were dark or nebulous and of a violet tinge; but in the November transits they were bright.

5. That if we take the two transits which have received the most careful observation (the May transit in 1832, observed by Professor Moll, and the November transit in 1868, observed by Dr. Huggins), we find the contrast very great and very typical—in the former a diffused spot preceding the centre, with dark ring surrounding the planet; in the latter a sharply defined spot following the centre, with a bright ring surrounding the planet.

The author therefore concluded that on May 6th next the luminous spot will not be a point of light having no sensible diameter, but rather diffused, with a periphery not well defined, gradually sinking from a greyish white to the dark colour of the disk, situated a little in advance of the centre and to the south of it, and that the planet will be surrounded by a dark nebulous ring, not a bright one.

The Astronomer Royal. Speaking in the interest of the Royal Observatory, I was somewhat surprised to find that there is no citation of the observations made at Greenwich in this account of Mr. Jenkins. Our observations get locked up to a certain degree in the volume published annually by the Observatory; but I should have thought that they would have been searched out, as there were observations in one instance certainly, and I think in two instances.

Mr. Chambers. I must express a sanguine hope that the 'Monthly Notice' containing this very interesting and useful paper will be published so as to be in our hands before the 5th of May next, if possible.

Prof. Cayley. It is almost an impossibility to bring out the 'Monthly Notices' sooner, even if it is a long month, as the last one was; but next month being shorter, I think it will not be practicable to bring it out much before the ordinary time, though I will do my best to do so as early as possible.

Capt. Noble. A well-read man, as Prof. Cayley is, will not need to be reminded of what the first Napoleon said about the word "impossible."

Mr. Chambers. Supposing it not to be possible to have the 'Monthly Notices' complete, so as to be in the hands of the Fellows by the time of the transit, it strikes me that Mr. Jenkins's observations are so eminently practical and so eminently likely to be useful to observers, that I venture to suggest that his paper should be separately distributed for the use, at any rate, of those Fellows who have observatories and telescopes.

The President. It would be perfectly within the power of the meeting to order that any paper of peculiar speculative interest should be printed and circulated amongst the Fellows, or amongst a certain number of them. I think in all probability it would be a very important paper to circulate. I deeply regret that I forgot to bring the matter before the last meeting, which I had intended doing, viz. to have asked that some of those gentlemen who had observed the transit of Venus in the Government parties should have met together, and drawn up a short table of recommendations to Astronomers in this country, as to the best method of taking observations of the transit of Mercury. If that had been done, the report or recommendations of the Sub-Committee might have been issued at the same time as Mr. Jenkins's paper. I believe the Superintendent of the American Nautical Almanack has issued a circular, but I have not had the pleasure of seeing it.

Mr. Ranyard. It does not contain suggestions with regard to physical observations like Mr. Jenkins's. It would also be of great interest if suggestions were made with regard to physical observations, especially before the planet comes upon the Sun as well as after it has entered upon the Sun.

Mr. Knobel. The circular issued from Washington with suggestions for observing the transit of Mercury contains merely suggestions to inexperienced observers, defining the method that they should adopt for securing the right time of the various phenomena; and it contains a diagram constructed to scale, so that they might observe to scale the apparent size of Mercury on the disk of the Sun, and mark the first contact with the disk according to the diagram.

The President. It seems to me a very convenient and practical thing, and well worthy of emanation from America; and it is a great pity that we did not get it beforehand.

Capt. Noble. We have a precedent for issuing things of this sort. I am an old Fellow of the Society, and I recollect that when a comet appeared we used to have little post-cards sent about. That seems to be a custom which is more honoured in the breach than in the observance now-a-days.

The Astronomer Royal. I may make this notification to the Society. The transit of Mercury comes on on the 6th of May. It so happens that, in consequence of the completion of the observations for the transit of Venus, and the return of a large part of the apparatus which was required for them, we have at our disposal a large number of telescopes at the present time. I am not able to allow them to go outside the precincts of the Observatory; but if any gentlemen wish to watch that portion of the transit which is observable in these localities, and should be disposed to view it within the Observatory, I can afford accommodation, I think, to about ten. We have at the present moment, besides the telescopes attached to our own staff, and which would be occupied by them, five refractors of 6 inches and the others 4 inches or thereabouts. If there are amateurs here who would be disposed to come to Greenwich to the Observatory grounds in such places as I can locate them in, I will do the best I can for them, if they will give me a fortnight's notice. I think there are several gentlemen present who are sufficiently familiar with the course of the phenomenon, and with what is requisite to make useful observations. I can give them outdoor accommodation for about ten telescopes, with a chronometer to each, with benches and tables and the like.

Mr. Beck. I think the remarks of the Astronomer Royal show how desirable he thinks it that these observations should be made, seeing that there is nearly a century before this occurrence will take place again. I should have been obliged if some of those who are so well qualified to give information could have issued a short pamphlet to the Astronomers in the country, because we know the uncertain character of our climate, and whilst the Sun may be obscured here, it may be all that may be desired for observation in other parts of the country. Such a pamphlet might be issued and sent to those who have instrumental means at their disposal to observe this interesting event.

The President. I think, if the Society will place confidence in me, I will endeavour to get up such a circular embodying the necessary information in a few short words, and making it as clear as possible to those who are not very well up in instrumental use. I can only say, on behalf of the Society, that I feel sure they will accord their warmest thanks to the Astronomer Royal for his most generous offer. If I were not going to Scotland for the purpose of observation, I should be amongst the first to ask for his accommodation.

The Astronomer Royal. I have had occasion very pointedly to remark the difference between observations made at two places actually within sight of each other. On the occasion of the Eclipse of 1842 I was enabled to see it very well, but my friend M. Plantamour, within sight of the place, did not see a particle of it.

The President. I can speak feelingly on that point, for when I was in Mauritius I went to that portion of the island which was supposed to be clear at all times. There was another portion of the island in which the Sun was scarcely ever seen for cloud. On my end of the island there was cloud pretty nearly the whole time. I had only 86 minutes of sunlight out of 4 hours; whereas in the other place, which was very nearly visible from the elevated plateau where I was, they saw the whole transit right through.

The thanks of the Society were unanimously voted to Mr. Jenkins for his interesting paper, and also to the Astronomer Royal for his generous offer.

Mr. Green. Professor Schiaparelli has sent me some very interesting drawings of the planet Mars; and I have also been favoured with two letters with four drawings of the planet. Both letters are on a matter of very great interest, being no less than the proposition that very great physical changes have taken place on the surface of the planet since the time of last opposition. The first letter from the Professor, after some pleasant complimentary remarks, mentions the appearance of several dark canals, as he calls them, through the lightest portion of the planet, which were not seen at the time of opposition, but became visible afterwards. The second letter bears so particularly on that point that I will give a rough translation of it. "I have received your esteemed letter of the 15th inst., for which I thank you. No doubt we have reason to be satisfied with the harmony which exists between our Mars researches. Your observation is very true that this harmony will be still greater when I endeavour to explain in my drawings the indefiniteness of the outline of the markings. An indefiniteness occurs which is unavoidable with optical instruments of an aperture which is limited; but I do believe that the harmony would not be greater if you had been able, as I was, to observe the planet in October and the following months. It was in October that the equatoreal zone of the planet began to show itself, and then it was that those narrow channels which intersect it began to be visible, some of them in a very remarkable degree. For instance, the channel which occupies as nearly as can be the place of Huggins Inlet in most pictures became so black and so bold, that I was able to see it very distinctly up to the 14th and 17th of March." The Professor does not state that these were drawings from the telescope, but they are the result of projection from a chart which he has taken four months to prepare from his various observations of the atmospheric effects. He goes on to say, "The channels which touched the planet's equator, and ranged from 55° to 170°, should be still visible. At any rate the first was seen here several times with great ease during the last week in February, and the other quite distinctly on Wednesday, March the 18th." I would make one remark as to the drawings; and I hope I shall be excused for the exercise of a little artistic criticism with regard to them, and that is, that the development of hard and sharp lines in them must be an error. These keen, sharp, round and exact lines are the result of the process through which the drawings have gone, and not the result of eyesight upon the planet. Again, there appears a tendency, either in the object-glass, in the eyepiece, or in the natural power of the eye, to elongate the development of certain dark points as seen against a light. We have that very clearly shown in the drawings of Mr. Dawes, and also in these very strongly-developed lines of the learned Professor of Milan; and a certain amount of light may be thrown upon that by a simple and emphatic statement, that on one occasion, at Madeira, when the planet was very clearly visible, I saw, or thought I saw, a line joining a certain small dark spot with what was previously known in Mr. Proctor's maps as the Sea of Lockyer; and I should have drawn it as a line connecting those two points, for I thought—and with all honour I could have drawn what I thought—I saw a connexion between them; but the evening cleared, the air became more steady, and I saw that those lines were not in any way connected, but were thoroughly separate, and therefore having drawn them as separate points in a clear atmosphere, and having seen them, as Prof. Schiaparelli drew them, with a small dark line joining them under a less perfect definition, I cannot but think that a great many of the clear hard dark lines drawn in many parts of Mars are the result not of perfect eyesight, but of imperfect eyesight, and the result of optical illusion. But I must speak of these drawings by Signor Schiaparelli with the greatest respect; for we have the greatest evidence in them of accuracy and clearness of vision and goodness of character, and a high amount of earnestness of research, for every mark in them, barring those particularly sharp lines, I can recognize in my own drawings; but when I come to them I find not the slightest trace of them was visible at Madeira during the opposition with an instrument of 13 inches aperture, as against the 8-inch object-glass at Milan. Now I think I may fairly pair a 4-inch object-glass against 5 inches of silver. I have done it very recently and conscientiously again and again for the purpose of testing a refractor against a reflector; and I had 13 inches of silver as against the 8-inch refractor. During the opposition none of those dark forms appeared; and therefore it must either be an optical illusion, or a most remarkable and extraordinary number of phenomena, or a great physical change on the surface of the planet during the months of January, February, and March. Not only long before but long after the next opposition, I shall keep my eye on the telescope, to see if I can support those lines of Prof. Schiaparelli's, and I shall be heartily rejoiced if I can.

Mr. Common. I should like to ask Mr. Green whether I understood rightly that those drawings are from observations of Mars made through a telescope.

Mr. Green. Not from the telescope; but they are the results of a projection on the map which Prof. Schiaparelli has made from his observations.

Mr. Common. Have you any of the original drawings from which the map was made?

Mr. Green. None whatever.

Mr. Common. There is a harshness of outline all over the drawings which does not harmonize with the view which you get of the planet, especially such as one might have obtained in the early part of this year.

The President. I think these drawings are merely the outcome of a series of sketches or physical pictures of what Prof. Schiaparelli saw in looking through his telescope, but massing together the various details which he saw at different times.

Mr. Common. It is of the greatest importance that we should have the original drawings, as every thing turns upon the fine lines which cross the planet at various points.

Capt Noble. I observed Mars most sedulously and carefully through the whole of the opposition with as fine a 4·2-inch refractor as exists in England, made by the late Andrew Ross. The drawings made by me through that opposition were laid upon this table; and I was going to ask whether any Fellow of the Society took them away. Mr. Wesley tells me that they were taken off the table the night that they were exhibited, with those exquisite drawings of Mr. Green's; than which nothing finer was ever shown in this room. I can only say that I never saw any thing of the wonderful reticulations which Prof. Schiaparelli shows.

The President. Prof. Schiaparelli is a man of extreme caution and care, and not likely to be carried away by his imagination; but I fancy there must be something peculiar in his telescope, or his eyepiece, or his eye, for I am sure that he would not make drawings if he was not fully persuaded in his own mind that they did exist.

Thanks were unanimously voted to Mr. Green for his interesting paper.

The President. A short time ago I received a letter from a gentleman directing my attention to a picture by M. Winkler, brother of the Astronomer at Weimar, exhibiting what he calls sunset in the Moon. This picture you see before you, in which the peaks of the mountains are seen brilliantly lighted up, the foreground being the floor of a crater. You will see there are many small craters, which, properly speaking, ought to be perfectly dark. In the absence of atmosphere in the moon, there would be no dispersion of light, and consequently any place where there could possibly be a shadow would be in utter darkness. The artist has very ingeniously utilized what may be called the earth-shine, and consequently certain half-tones, which are visible, are got from the reflection of the earth. Rather towards the right is the star Antares with Scorpio, and the Milky Way is shown in the background. It is such a remarkable picture, that I took the liberty of asking that it be sent here this evening, because I thought it would interest some of the Fellows.

Mr. Marth. It strikes me that the picture is in one respect an impossible one. The light comes from below the ecliptic.

The President. Mr. Winkler was assisted all through by Prof. Brühns.

Mr. Marth. The Earth, judging from its contour, is about eight or nine days old, so that the Sun must be distant 105° or 120°, and the Earth ought to be turned round to suit the stars. Besides, from which part of the Moon is this taken? It seems to me that Scorpio cannot appear so while the Earth is shining as shown in the picture. The size of the Earth seems correct. In pictures at the Royal Academy the painter usually does not represent what he has seen of the Moon; but she is painted far too large.

The President. The artist has nowhere stated whether the picture is taken from this side of the Moon or the other. It is a purely imaginary landscape.

The Astronomer Royal. I think we are trifling with the time of the Society. We know what the Moon does look like as seen in a telescope.

The President. This picture was undertaken after the artist had read Mr. Nasmyth's and Mr. Carpenter's book on the Moon; and it is a sort of attempt to realize on canvas the drawings prepared for the model casts by Mr. Nasmyth, of which we have an example on the walls of this room.

Capt. Noble. You said that on account of the absence of atmosphere in the Moon every thing which is not in sunlight would be dark; but it is quite possible to conceive that, if there were a mass of rock facing the Sun, the light might be reflected into the plain beneath sufficiently to render it visible, without atmosphere at all.

Mr. Chambers. It seems to me that, according to Capt. Noble's explanation, the sunlight must come through the rock in order to illuminate the crater.

Mr. Penrose. There cannot be a moment's doubt that the power of reflection from the large surface of a mountain in the Moon would give a reflection to other parts which do not receive the sunlight, because the dark grey part of the Moon is visible by the Earth's reflection, and therefore portions of the Moon might be visible from the nearer sunlight on a lunar mountain. But certainly the crags in the picture are not at all in accordance with the shape of the lunar mountains and ridges.

Mr. Ranyard. I imagine, if we were in the Moon, and looking at a star without an atmosphere, we should see a very great mass of smaller stars. In the picture there are very few stars below the fifth magnitude; and the Milky Way is given very faint. I cannot quite recognize what parts of the Earth are looked at; certainly no point of specular reflection is given there. I imagine we should see a more conspicuous fringe at the terminator than at the limb when the light is falling nearly perpendicularly from the Sun. The illumination of the clouds within an atmosphere must be very strong, and therefore we should not expect to find the blue part of the spectrum thinned out as we should towards the terminator; and we should expect a less roseate appearance upon the limb than upon the terminator.

Mr. Marth. Perhaps I may be allowed to come back to Astronomy again. As the Astronomer Royal mentioned, we know very well how the Moon looks; yet we have no representation of our Moon as seen in a very small telescope. What we want is a drawing of the full Moon, looked at with a very small power of 2 or 5. It is almost essential that draughtsmen should do this simple thing, that we may judge of the value of their drawings, of Mars for instance. May I ask whether anybody who has observed Mars has ever taken the trouble to determine the exact time of the transit of spot *a* across the central meridian? Up to this time I have only received a single observation, by Mr. Green, who has estimated the passage of the spot *a* on one day, but the limits amount to something like ten minutes. If several observations of the passage of spots had been made it would be possible to determine the time of rotation with a very great degree of nicety. Although I have very great confidence in Mr. Green's accuracy, yet if we could have several observations, they might check one another. The limiting values found from Mr. Green's observation, are $24^h\ 37^m\ 22^s{\cdot}66$ and $24^h\ 37^m\ 22^s{\cdot}64$, which approximate more closely to Kaiser's value, $24^h\ 37^m\ 22^s{\cdot}62$, than to Proctor's, $24^h\ 37^m\ 22^s{\cdot}73$.

The following papers were announced:—

R. P. Greg. On the duration of Meteor Showers.

E. J. Stone. A comparison of the Observations of contact of Venus with the Sun's limb in the Transit of Venus of 1874, December 8th, made at the Royal Observatory, Cape of Good Hope, with the corresponding Observations made at the Stations in the Parliamentary Report, and a discussion of the Results.

J. R. Eastman. Observations of the Companion of α Canis Majoris with the Transit-Circle of the U.S. Naval Observatory.

Rev. S. J. Johnson. Early Transits of Mercury.

Lord Lindsay. Note on the Table p. 70 of Vol. II., Dun Echt Observatory publications.

The following were ballotted for and duly elected Fellows:—

Camille Flammarion and Alfred J. P. Shepherd.

Celestial Photography[*].

In 1850, the Director of the Harvard Observatory, Mr. William C. Bond, engaged the photographer Mr. John A. Whipple, of Boston, to conduct a series of experiments with the great telescope of that observatory, which possessed the largest object-glass then in existence, except that of its twin instrument at Pulkowa in Russia, the diameter being 38 centimetres. Several photographic impressions of the Moon were then taken, and images also obtained from the stars Vega and Castor. The latter star is double, and the image appeared elongated.

These appear to have been the first lunar and stellar photographs to which publicity was given; but it has since been stated that images of the Moon upon silvered plates were obtained by Dr. John W. Draper in New York, some ten years previously. I may also mention that photographic impressions of the Sun were taken at intervals during the progress of the solar eclipse of 1854, May 26, under the direction of Prof. Bartlett at West Point, this being, so far as I can learn, the first occasion on which photography was actually employed with success for purposes of celestial observation.

In 1857 Mr. Whipple obtained with the Cambridge telescope impressions of the stars 'Alcor' and 'Mizar' in the Great Bear, the latter being a double star, whose components are 14½ seconds distant from each other, and of the third and fifth magnitudes respectively. Memoirs were published on the subject by Mr. George P. Bond, subsequently himself director of the same observatory, who made numerous measures of the distance, and three of the position-angle of the components.

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[*] Address of Professor Gould on receiving from the Governor of the Province of Cordoba the premiums awarded at the Centennial Exhibition in Philadelphia to the Argentine National Observatory and to himself, for Lunar and Stellar Photographs.

In the use of ordinary telescopes for photographic purposes a serious difficulty is encountered. The large instruments in general use at observatories are achromatic refractors; that is, they are telescopes in which the light from the object viewed is received through a compound lens formed of single lenses of different densities whose curves are so contrived that most of the visual rays, although differing in refrangibility, shall be essentially brought to one common focus. But those rays in the light, which ordinarily serve for vision, are not those which possess the greatest chemical power; while it is upon this latter class that we must necessarily rely for photographic impressions. What we require is an object-glass which shall concentrate the most effective chemical rays into a common focus, in the same way that the achromatic lens concentrates the visual rays.

This difficulty is avoided when a reflecting telescope is used; for in this class of instruments the mirror which receives the light reflects all its component rays, of every sort, to one common focus. For this reason it has been found convenient to employ reflecting telescopes for photographs of celestial objects; and this motive has stimulated to essential improvements in this class of instruments. Admirable lunar photographs were thus made at an early date by Drs. H. Draper in New York and W. De La Rue in London, and with the magnificent telescope under the direction of Mr. Ellery at Melbourne, which has a speculum of 4 feet in diameter.

But here another class of difficulties is encountered, since the reflecting telescopes are of far less convenient management than the refractors. Not only does their form render them more cumbrous and unwieldy, but much larger dimensions are needful to insure equal optical power; while at the same time constant care is requisite to preserve the reflecting surface in good condition.

A happy device of Mr. Lewis M. Rutherfurd has greatly diminished the obstacles to the employment of refracting telescopes. It is evident that could we construct an object-glass which should concentrate the chemical rays, instead of the optical, as I have already described, the desired end would be attained. But how to accomplish this?—since the forms of the curves in an achromatic object-glass are guided and corrected during its construction by means of optical tests, which are inapplicable for the chemical rays. This problem was solved by an adroit application of the spectroscope, by means of which Mr. Rutherfurd managed to test and perfect the surface of what might by analogy be termed a chemically achromatic object-glass, and with such success that lenses of this kind can now be constructed with almost the same facility as an ordinary object-glass. Such lenses are of course useless for vision, but they afford the greatest possible advantages for photography.

The numerous practical difficulties having been surmounted once for all, Mr. Rutherfurd completed a lens of this sort in 1864. Its diameter was 28½ centimetres, and early trials showed it to be capable of affording photographic images quite equal in strength and sharpness to those of the fine reflector then recently constructed by Dr. Draper with a speculum of silvered glass 2 feet in diameter.

With this lens he obtained a large number of photographs of the Sun, Moon, and prominent Star-groups with unexampled success. The evenings of March 5th and 6th, 1865, were of unusual clearness, and the photographs of the Moon near her first quarter made on these evenings will not soon or easily be surpassed. Impressions from these are well known throughout the scientific world, and have commanded universal admiration.

The images of star-clusters, on the other hand, possess no popular attractiveness. They are but black spots upon the albuminized surface of glass plates; and their value consists solely in the accuracy with which the relative positions of these several dots may be measured. But this is no slight value; for if all precautions have been taken to prevent undue distortion, the photographic record may be subjected to repeated measurement and kept through future years available for new examinations, whenever desired, and for new comparison with the heavens after the lapse of centuries, so as to permit the detection of whatever changes of position may occur among the several stars.

To facilitate the measurements, Mr. Rutherfurd also constructed some admirable micrometers, furnished with strong microscopes, by means of which the relative positions of the stars upon very many plates have been carefully determined by himself or under his direction, and the accuracy of the measurements thus made seems to surpass that of any other micrometric method. Photographic images, impressed upon the plate in a few minutes on some especially clear night, offer material for hundreds of hours of laborious measurement. And these measures, instead of being restricted to favourable nights, at favourable seasons, and painfully made by the astronomer in inconvenient postures and under all the attendant disadvantages, may be easily performed at any time and place, even in another hemisphere, with all the convenience and comfort which the nature of the case admits, and subject to indefinite repetition. Sufficient material to occupy all the energies of an astronomer for a year or more may thus easily be collected in a single night and reserved for subsequent study.

Early in 1866 Mr. Rutherfurd kindly placed at my disposal the results of repeated measurements of stars in the Pleiades, from many different photographs taken during the previous year; and I had the gratification not only of deducing from them the right ascensions and declinations of nearly fifty stars in this cluster, but also of convincing myself of the smallness of the average error of a simple determination, whether as regards the discordance between different measures made upon the same plate, or that between the positions of the images upon different plates. In August of that year I communicated the results to the National Academy of Sciences, and sent a short communication containing the resultant positions for the ten brightest stars of the group to the 'Astronomische Nachrichten' of Altona. The results not only proved the extreme accuracy of the determination made by the great astronomer Bessel more than a quarter of a century before, but likewise established the important fact that there had been absolutely no essential change in the relative positions of these stars during the intervening period.

In 1870, shortly before coming to this country, I made a similar series of calculations of the position of stars in the great cluster called the Prœsepe in the constellation Cancer, from measurements also furnished me by Mr. Rutherfurd, and taken from his own plates. I also left in the hands of this friend a memoir on the subject to be published at a fitting time, since a publication of the results of these computations, previous to any regarding the observations themselves by their author, describing the photographic methods which he had employed and the construction of his micrometric apparatus, would have been in the worst of taste.

But I had learned enough of the beauties of the photographic method of observation to become enamoured of it, and enough of its capabilities to give it my fullest confidence. The various objections urged against it at the beginning had been proved unfounded: experiments had shown that little or nothing is to be feared from shrinking or warping of the collodion film, and that even the great diversity in size of the images for bright and for faint stars is not incompatible with micrometric comparisons quite as accurate as, if, indeed, not more so than those made with the telescope directly. And I cherished the fervent desire to secure a series of photographic impressions of the Southern clusters, which might afford material for study in later years.

Circumstances seemed to favour this plan. Mr. Rutherfurd had just procured a new telescope, with an object-glass of 33 centimetres, being one third part more powerful than its predecessor, and had availed himself of the opportunity of adapting it to photography by a new method, viz. a correcting lens, which, being applied to the achromatic object-glass, produces a compound lens similar in its action to those constructed for photographic purposes only. The former smaller lenses were received by the makers in part payment, and I was thus enabled to secure the identical glass with which such splendid results had already been obtained.

The totally different aims which prompted my coming to this hemisphere, and to which I purposed directing the energies of the new Argentine Observatory, together with the slender resources available for its construction and maintenance, made it impossible to divert any of its funds to photography; but on making my desire known, sundry of my kindred and personal friends assured me that the pecuniary means should not be wanting, and encouraged me to go forward with my plans. There was an additional propriety in making it a private undertaking inasmuch as there was small probability that the tedious measurements of the photographic plates could be executed during my sojourn in this country, which I supposed would be far shorter than the event has shown.

I consequently enlisted the friendly services of Mr. Rutherfurd in behalf of my plan; and received from him the ready assurance that, whenever I desired, he would engage for me some practical photographer, and give him in his own private observatory the special instruction and practice needed for the application of his art to astronomical purposes. On arriving at Buenos Aires I made known my hopes and plans to President Sarmiento and to our present chief magistrate, Dr. Avellaneda, then Minister of Public Instruction, and found with both of them such encouragement that, as soon as the Observatory had been erected and the telescope mounted, I made official application for leave to use the telescope for my own photographic purposes, whenever this might be done without conflicting with the regular series of observations. My request being cordially granted, I wrote to Mr. Rutherfurd, asking him to engage for me a private photographic assistant, and to purchase the necessary chemicals and apparatus.

The original plan for a subscription of twelve thousand dollars in Boston for aiding my project had not been carried out. Postponed at first because not needed immediately, it had been impeded by the grave financial crisis and other public calamities which occurred soon afterwards. But the surviving parents of myself and my wife begged me not to be deterred, and offered me the requisite means from their own personal resources—a generous offer which I declined accepting, but which justified me in going forward with my plans, since I might rely upon this future support in case of emergency.

Thus in March 1872 a photographer arrived, after due training and instruction in Mr. Rutherfurd's Observatory, and brought with him a full supply of the various apparatus and chemicals needed. I constructed a small laboratory in the observatory building, gave what additional instruction was necessary, and when all else was ready, unpacked the photographic lens, which had lain until then undisturbed in the box in which it had come from the United States.

My hearers already know the terrible disappointment which awaited me:—I found the flint glass of the lens completely broken into two nearly equal parts. How or when this happened I know not, nor would investigation help the matter. It had not been subjected to the risks of ordinary transportation, but had been brought by hand, like a chronometer, in a box especially made for the purpose.

Lamentations being useless I did the best that I could. Attempts at cementing the fragments with the needful precision proved fruitless; but at last our skilful townsmen M. M. Perrin constructed from drawings, made by Mr. Davis, an apparatus in which each piece of the broken lens was held in place by three pairs of screws, so arranged as to permit a very delicate adjustment of its position. The object-glass was thus reconstructed with sufficient nicety to give moderately good photographs of the moon and impressions of a few star-clusters. Meanwhile I took steps to secure another photographic object-glass, calling upon my mother for the means of defraying the necessary expenses. Mr. Fitz undertook the construction, Mr. Rutherfurd kindly giving a general superintendence and calculating the surface-curves corresponding to the disks of glass which were available. Here, too, the National Government once more afforded its generous support, by giving me, before the lens could be completed, full authority for its acquisition by the Observatory itself, together with an assurance that the aid of a photographic assistant should thereafter be provided without further personal outlays by myself.

This new object-glass reached Cordoba about the middle of the year 1873; but then followed the new series of trials and disappointments with which many of my audience are well acquainted and which I will not here detail, since they have been sufficiently alluded to in my annual report for that year. Suffice it to say, that the photographer proved utterly unworthy; the time, apparatus, and materials I had provided for stellar photography were consumed by him in making photographs of the moon and terrestrial objects, which were disposed of for his personal advantage and emolument; so that at the close of three years' struggle I found my resources diminished by several thousand dollars, with absolutely no return excepting a few indifferent photographs, of which, I grieve to say, the larger part have since become useless by the peeling of the collodion, which had not been applied with sufficient care. It is a source of regret to me, moreover, that I am not the only person, nor the Observatory the only institution, that has suffered through the same agency.

The sacrifices already made, and the long series of severe disappointments and mortifications already suffered, forbade me to think of desisting from the cherished scheme, and on my return to the United States in 1874 I improved the opportunity to engage another photographic assistant, Mr. John A. Heard, to whom I am largely indebted for our final success. He had already obtained repute as a skilful manipulator, and he, like his predecessor, passed some time at Mr. Rutherfurd's Observatory, learning the chief peculiarities of astronomical work. He arrived in Cordoba early in 1875, and remained until his health required him to return home. The National Government, with that same readiness to support me which has never failed whenever circumstances permitted, provided the salary for a regular photographic assistant; and although I am just at present without aid in this department, I am hopeful that Mr. Heard's health may soon be sufficiently restored to enable him to return, and am confident of improving upon the results heretofore obtained.

Such is the history of this new method of observation and the efforts made to advance it in Cordoba. With the quality of the results obtained I feel satisfied. All the photographs have been made upon plates of glass 12 centimetres long by 9 broad, the images of the moon being 3·52 centimetres in diameter before enlargement. The photographs of the moon at the full and in the last quarter may, I think, be favourably compared with any I have seen of these phases; and the enlargements, 48 centimetres in diameter, being printed in carbon, can never fade. Mr. Rutherfurd's photograph of the first quarter is more sharply defined than ours, and will probably long continue to claim the palm. A very beautiful picture of the moon, made with the great 4-foot reflector at Melbourne, was also exhibited in Philadelphia. This represents her, not at the quarter, but with about nine days' illumination—thus showing a very considerable portion of the lunar surface with the oblique light, which is so desirable for artistic effect, as well as for the proper contrast of mountain and valley. Had I seen this elegant photograph before placing our own on exhibition, I am not sure that I should have ventured to compete.

Much of the credit of the stellar photographs is due to the pure air of Cordoba, which is incredibly transparent on those not very numerous occasions when the sky is truly clear. The impressions on glass which I exhibited were of six different clusters—the plate of the cluster *X Carinæ* containing two images each of 185 stars, and that of *Eta Argus* (or *Eta Carinæ* as we now call it) containing 180. I think there can be no reasonable doubt that many of the stars there depicted are quite as faint as the 9th magnitude, and have the satisfaction of adding that upon other photographs, obtained since then, there are images of stars much fainter still; but how much, I am as yet unprepared to state. Since it is important that all the images should be perfectly round, without the slightest perceptible elongation, and since each of the two exposures should sometimes be for as much as 10 minutes, during which period the motion of the telescope must not deviate appreciably from that of the star in its diurnal motion, the prepared collodion thus requiring its full sensitiveness for something like 22 minutes in the excessively dry atmosphere of Cordoba, it will be seen that the problem has not been without its difficulties.

We have already secured measurable photographic impressions of not less than 84 celestial objects, of which 19 are double stars and the remainder clusters; so that, even should the undertaking remain paralyzed in its present state, the results may be regarded as richly worth the labour, money, and perhaps even the other sacrifices that they have cost. I may add that the planets *Jupiter, Mars,* and *Saturn* have likewise been photographed with sufficient distinctness to show clearly the details of light and colour on the surfaces of the two former, and the existence of the ring in the latter; but these images have not been sufficiently sharp to permit of successful photographic enlargement.

B. A. Gould.

Average per cent. of the recorded Magnitudes of Shooting-Stars.

Mr. E. F. Sawyer's interesting note on this subject ('Observatory,' No. 12, p. 399) has caused me to count and sort out, according to magnitude, a large number of meteors in various British and foreign catalogues, including the observations of Heis, Weiss, Zezioli, Denza, Schiaparelli, Konkoly, Tupman, Lucas, &c., and I have drawn out a table of the percentages deduced therefrom. Dr. Schmidt's and Mr. Sawyer's observations are included, also Mr. Corder's results. The whole of these collected materials include 57479 shooting stars.

Observer.	Years of Observation.	Percentage of Magnitudes.					Total Number of Meteors.
		Class I. >1st mag.	Class II. =1st mag.	Class III. =2nd mag.	Class IV. =3rd mag.	Class V. =4th mag. & under.	
J. F. J. Schmidt	1842–68	3·8	14·6	18·0	22·3	41·3	1666
P. F. Denza and others	1869	2·4	8·4	13·2	32·0	44·0	1007
G. Zezioli	1867–70	*11·5		20·8	27·1	40·6	8392
E. F. Sawyer	1868–77	2·8	10·7	17·0	25·4	44·1	6925
H. Corder	1871–78	*8·3		18·4	29·6	43·7	3162
G. L. Tupman	1869–71	3·6	11·2	15·5	24·7	45·0	1836
W. F. Denning	1876–77	2·9	7·3	19·5	24·7	45·6	2894
N. de Konkoly	1871–76	4·5	14·5	21·3	24·9	34·8	2189
G. V. Schiaparelli and others	1872	3·1	12·7	22·0	25·1	37·1	7063
E. Heis	1833–75	2·1	19·3	35·7	28·5	14·4	13919
E. Weiss	1867–74	1·9	11·5	24·6	32·3	29·7	6013
J. Lucas	1869–77	3·5	21·1	26·8	28·0	20·6	1846
Tanger and Stephens	Aug. 10, 1871	*9·2		9·2	16·3	65·3	567
Average	1833–78	2·8	11·9	20·2	26·2	38·9	57479

The table exhibits very large differences between the several observers. The first nine, however, agree very fairly well; and taking an average of these we have:—

	>1st mag.	=1st mag.	=2nd mag.	=3rd mag.	4th mag. & less.	No. of Meteors.
Per cent.	3·0	10·6	18·4	26·2	41·8	35134
Increase per cent.		7·6	7·8	7·8	15·6	

which very closely corresponds with Mr. Sawyer's averages given in his letter above referred to.

Dr. Heis's magnitudes appear to be much overestimated. Though he recorded nearly 5000 of the 2nd magnitude, he only has 4000 of the 3rd magnitude, and less than 2000 of the 4th magnitude and under. The explanation appears to me to be that his registered meteors were selected on account of brightness; for he made nearly all his observations during the latter half of July and first half of August, when the *Perseids* and some other showers being very plentiful, readily enable an observer to make such selections, and still record a large number of paths. I do not see how the excess can be otherwise accounted for in an observer of such experience and ability as Heis, accustomed as he was to the estimation of star magnitudes. He registered a large number of faint meteors (4th, 5th, and 6th magnitudes) in the years 1847–50, but after that there are very few indeed less than 3rd magnitude. At the same time, Dr. Weiss s observations in the table are rather high; and Mr. Lucas's at the Radcliffe Observatory the same, especially in Class II., of which the percentage is greater than in Class V. No doubt the explanation is the same as that applied to Heis's results; Mr. Lucas made nearly all his observations during the annual returns of the *Perseids*, when many bright meteors may be registered. Even when imperfectly seen, fireballs and meteors of the larger class are invariably recorded, while fainter ones are allowed to escape; and this method of selection no doubt affects more or less all the observations the results of which are summarized in the table. The vast number of records I have been examining contain many isolated accounts of fireballs; and it is certain that, these being included, the proportion to the fainter classes of meteors observed during habitual watches is too large. The observations of the *Perseids* on Aug. 10, 1871, by Messrs. Tanger and Stephens give a large percentage of faint meteors; but these are the *actual observed* magnitudes, and are therefore of more value than the *registered* magnitudes.

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* These figures are included in the average results by dividing them according to the mean of the other observations.

As to the remaining values, they agree closely, and there appears to be a regular definite increase of about 7·8 per cent., as pointed out by Mr. Sawyer from his own observations. In this connexion it should be explained that class V. contains both the 4th and the 5th magnitudes, and a few of the 6th also.

The percentages deduced from the 13 sources are accurate only as representing the *registered* magnitudes, and cannot be held to show the *actual* relative values. The first 9 catalogues in the table give results that are probably far nearer the true proportion of percentages, though these are also no doubt affected to a certain extent by the disposition of observers to record the brighter meteors. The figures are therefore presumably too high for the greater magnitudes, and too low for the fainter ones in regard to the actual relative percentages of the visible shooting-stars. But the correction required is probably very small, and chiefly affects classes I. and V.

It would be interesting to compare other results with these now given. The Italian observers have amassed an enormous number of unpublished observations, and there must be other catalogues that I have never seen.

Ashleydown, Bristol,
April 15, 1878.

W. F. Denning.

CORRESPONDENCE.

To the Editor of 'The Observatory.'

Spiral Form of Nebulæ.

SIR,—

In reply to my remarks published in No. 11 of 'The Observatory,' M. Tempel finds it strange that I should dispute his criticism of some drawings made with Lord Rosse's telescope, as I had a year ago in a private letter approved of his intention of criticizing them. When I wrote that letter I could not expect that M. Tempel would go so far as to deny the existence of spiral nebulæ altogether, apparently on no other grounds than his inability to recognize them as such in his 11-inch refractor. M. Tempel also finds my remarks on M. 51 to be "uncalled for," as he had not made any allusion to this object in his article in the 'Astronomische Nachrichten.' But as M. 51 is the most striking instance of a spiral, and as M. Tempel, in some notes on nebulæ which he kindly sent me for my supplement to the 'General Catalogue,' expressly had stated that he did not see M. 51 as a spiral, I considered myself justified in mentioning this nebula.

And now with regard to my admission in the private letter to M. Tempel of the superiority of Lassell's drawing to the one published in the Phil. Trans. for 1850, no one acquainted with the appearance of this difficult object in a large reflector will think it remarkable that Mr. Lassell's two drawings disagree in the representation of the arm starting from the north side of the large nucleus. The beginning of this arm is extremely difficult to see; and as it joins the other arm near the star No. 12, the commencement of the first arm is not well brought out, except with a fresh speculum and on a good night. I can therefore easily satisfy M. Tempel's curiosity as to which of Mr. Lassell's drawings I prefer; the first one (fig. 27) gives the best general idea of the appearance of the nebula, but at the same time I can perfectly understand Mr. Lassell having seen it on another occasion as shown in fig. 27 A. When Part II. of the Birr observations of nebulæ has been published, M. Tempel will have an opportunity of forming a better opinion of the appearance of M. 51.

Lastly, M. Tempel makes some remarks on my allusion to D'Arrest as an experienced observer who never doubted the reality of spirals; and he says with great triumph that there is not a single word in D'Arrest's work to prove his ever having seen a spiral. It is a pity that M. Tempel did not take the trouble to read my letter in 'The Observatory' more carefully, as my next remarks about D'Arrest having acknowledged the superiority of large reflectors in this respect would not then have escaped his notice, and he would have written his answer with that calm reflection which he regrets not to have found in my letter.

There is one point on which I shall trouble you with a few lines more, namely, the opinion of D'Arrest on the relative merits of an 11-inch refractor and a 6-foot speculum in good order. This great master in the realm of nebulæ (as M. Tempel justly calls him) is dead and cannot state his opinion himself; but I can bear testimony from a letter written by him to me on Nov. 13, 1874, of which the following is a verbal translation:—"My splendid telescope is, however, immensely inferior to Rosse's, so that I, although I certainly have tried to make the utmost out of it, cannot at all bear witness against what you see with the 6- or the 3-foot mirrors." After having alluded to a new work on nebulæ which had just been published, he continued:—"But I am very glad that Lord Rosse and you seem more and more to return to the nebulæ. Here every single observation made with the Rosse telescopes is a step forward which nobody else is able to make now-a-days. You have there no competition at all to contend with; on all other subjects we others can also say a word."

D'Arrest's ideas of the 6-foot telescope may or may not have been exaggerated; but as his opinion must be of some value I have thought it useful to quote it here. That many nebulæ may be drawn well with a good 11-inch refractor is still my opinion, and nobody will be more delighted to see M. Tempel's drawings published than I will.

I am, Sir, yours faithfully,
J. L. E. Dreyer.

Observatory, Birr Castle,
1878, April 5.

Colours of Meteors.

Sir,—

I have recently been looking over my list of meteor observations since 1871, to collect all the notes of the colour observable in these bodies, with the following result. Out of 3334 available observations, 225 had a colour assigned to them.

I have arranged them also according to their magnitudes.

	= ♀ - Sirius.	= 1st–2nd magn.	= 3rd–6th magn.	Totals.
Red..............	1	12	9	22
Orange	8	101	26	135
Yellow	3	25	2	30
Yellow-green...	1	2	0	3
Green	9	1	0	10
Bluish green ...	3	1	0	4
Blue	3	9	4	16
Mauve	5	0	0	5
Totals	33	151	41	225

From this it will be seen that although orange so greatly predominates in the moderately bright ones, it is rather rare in the fire-ball class, which is characterized by green and mauve.

The following table is made up from observations of other observers, including the list of large meteors in the British Association Report for 1876:—

	> Sirius.	= 1st–2nd magn.	= 3rd–6th magn.	Totals.
Red and Orange	14	22	17	53
Yellow	0	3	1	4
Green................	13	1	0	14
Blue	8	5	0	13
Violet...............	1	0	1	2
Totals	36	31	19	86

These agree pretty well with my own observations, but I kept them separate, as it is difficult to tell sometimes what colour is meant, especially when the same meteor is called blue, green, or red.

I suspect that some of the blue ones in the latter table I should have classed as mauve, by which colour is meant that peculiar tint obtained in pyrotechny by the use of copper.

The Perseids are an interesting family as regards colour. The largest that I have seen, which are curious broad flashes, are pale green, the brightest 1st mags. yellow, and a great part of the 1st and 2nd mags. rich orange.

The Geminids rarely exhibit traces of colour, but when they do they are extremely beautiful. The brightest 1st mags. very beautiful pale green, the next size smaller lemon-yellow.

The trains and streaks also vary in colour, but the former are generally red or orange. A slow-moving meteor from Perseus in Jan. 1877 had a pale primrose nucleus, with a crimson train. Green meteors, I believe, commonly have reddish trains of sparks.

I am, Sir, yours truly,

Writtle, 1878, April 5.

H. Corder.

Meteor.

Sir,—

My brother and I observed an unusually fine meteor last evening. It appeared at a somewhat low altitude, but I think the following path is fairly correct:—from R.A. 42°, Dec. 30° N. (N.E. of 39 Arietis), to R.A. 47°, Dec. 20° N. (near ζ Arietis). At first it seemed merely a small streak-like meteor, but growing larger and larger, before half the apparent course was accomplished it had become a bright round ball, of a yellowish-red hue, about one third the size of the moon, and much more bright than Sirius; the small streak, or tail, which was seen at first faded away when the meteor attained its greatest brightness, and it proceeded and finally disappeared without giving off any sparks &c. or bursting. We could not catch any sounds of explosion, but noises near might have prevented this. Duration of visibility was about two seconds.

I am, Sir, faithfully yours,

Rayner's Road, Putney,
1878, April 21.

James L. M'Cance.

NOTES.

On the Reversal of the Lines of Metallic Vapours *.—In a paper recently read before the Royal Society (Feb. 1878) Profs. Liveing and Dewar gave particulars as to some experiments on the reversal of the lines of metallic vapours. They made use of iron tubes about ½ inch in internal diameter, and about 27 inches long, closed at one end, and coated outside with borax or a mixture of plumbago and fire-clay. The tubes were inserted nearly vertically in a furnace fed with Welsh coal, and the observations were made through the open end of the tube. To exclude oxygen and avoid variations of temperature, hydrogen was introduced in a gentle stream by a narrow tube into the upper part only of the iron tube, so that the hydrogen floated on the surface of the metallic vapour without producing convection-currents in it.

By these means the following observations have been made:—

The characteristic lines of thallium, indium, and magnesium were seen reversed; the lithium line was seen reversed, but only in a mixture of the vapours of potassium and sodium.

The absorption-spectra of sodium and potassium were observed; and Profs. Liveing and Dewar remark that there is a great deal yet to be observed in regard to these spectra. Up to the present time they have not observed any of the appearances noted by Lockyer in the Proc. Roy. Soc. Vol. xxii., but they have noted the channelled-space spectrum of sodium described by Roscoe and Schuster in the same Vol. of 'Proceedings.'

Profs. Liveing and Dewar remark that the absorption spectrum of sodium vapour is not so simple as has been represented.

"The fact that the vapour of sodium in a flame shows only the reversal of the D lines, while the vapour volatilized in tubes shows a channelled-space absorption corresponding to no known emission spectrum, appears to be part of a gradational variation of the absorption spectrum, which may be induced with perfect regularity. Experiments with sodium carried out in the way described exhibit the following succession of appearances, as the amount of vapour is gradually diminished, commencing from the appearance when the tube is full of the vapour of sodium, part of it condensing in the cooler portion of the tube, and some being carried out by the slow current of hydrogen. During this stage, although the lower part of the tube is at a white heat, we have always noticed, as long as the cool current of hydrogen displaced metallic vapour, that on looking down the tube it appeared perfectly dark. The first appearance of luminosity is of a purple tint, and with the spectroscope appears as a faint blue band commencing with a wave-length of about 4500, and fading into the violet. Next appears a narrow band in the green with a maximum of light, with a wave-length of about 5420, diminishing in brightness so rapidly on either side as to appear like a bright line. This green band gradually widens, and is then seen to be divided by a dark band with a wave-length of about 5510. Red light next appears, and between the red and green light is an

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* Abstract of paper read before the Royal Society (Proc. Vol. xxvii.).

enormous extension of the D absorption-line, while a still broader dark space intervenes between the green and the blue light. The dark line in the green (about 5510) now becomes more sharply defined. In the next stage the channelled-space spectrum comes out in the dark space between the green and blue and finally in the red. Gradually the light extends, the channels disappear, the D-line absorption narrows, but still the dark line in the green is plainly discernible. Lastly there are only the D lines of absorption. So far as we can judge, the blue and the streak of green light at first observed are due to luminosity of the vapour itself where it is somewhat cooled, the later stages being mixed phenomena of absorption and emission."

The absorption-line about 5510, which has not been distinctly recorded by other observers, has been traced under various conditions.

In the case of potassium vapour, whether observed in the iron tube or in a glass tube, an absorption-line was seen with a wave-length of about 5730, which does not correspond to any known bright line of that metal. Profs. Liveing and Dewar point out that the method may be used for observing emission as well as absorption spectra; and they reserve for future investigation and communication the discussion of the identity or non-identity of these absorption-lines with lines in the solar spectrum.

THE SATELLITES OF MARS [*].—In a memoir published in 1849, M. Roche showed that a satellite of the same density as its planet could not exist in the ellipsoidal form at a less distance from the primary than 2·44 radii, thus explaining the existence of Saturn's rings, which are too near the planet to allow of their agglomeration to form a satellite. The first satellite of Saturn is at a distance of 3·35 radii, well beyond the limit, as determined by M. Roche. The inner satellite of Mars is distant 2·7 from the planet, and is therefore also outside the limit, though very close to it; so that there is no difficulty in this case either. But the remarkable fact that the period of revolution of the inner satellite is much less than that of the rotation of the planet is opposed to the cosmogony of Laplace, which assumes that the rings thrown off one after the other by a spheroidal nebula as it cools, condense in the plane of the equator at the limit where the centrifugal force of the rotation exactly balances gravity. In fact the law hitherto laid down has been, that "satellites are all situated beyond the limit which corresponds to the centrifugal force of the primary." M. Roche, however, points out that he showed in 1853 that half the ring of Saturn is within this limit, which for Saturn is nearly twice the radius of the planet, and that he was thus led to modify Laplace's theory, by recognizing the existence of interior rings distinct from Laplace's equatoreal zones. According to M. Roche's views, the portion of the nebula which becomes free at each new stage in the process of cooling consists of the whole of a fluid envelope which reaches to the poles. This fluid runs off on both sides towards the equator and streams away by the

[*] Comptes Rendus, Vol. lxxxvi. No. 7.

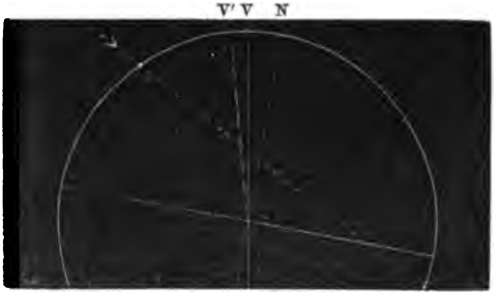
equatoreal line as by a sort of opening. Thus, on flowing to the equator, part of these nebulosities arrive there with a velocity insufficient to keep them revolving in exterior orbits ; instead, then, of separating from the nebula they enter its atmosphere again, and if this is sufficiently rarefied may exist in it for some time under the form of elliptic trains having their aphelion at the equatoreal limit. Such of these trains as have a small eccentricity transform themselves gradually, under the influence of the medium circulating round them, into a ring or assemblage of rings. Such is the probable origin of Saturn's rings. In the Saturnian system the trains revolving in the neighbourhood of the planet have remained in the state of cosmical dust and have preserved the form of rings, without being able to agglomerate into a spheroidal mass, because their distance from Saturn is less than 2·44 radii, the system of rings, in fact, extending from 1·5 to 2·4. For the interior rings the period of rotation must be less than that of Saturn, 10^h 14^m. The period 10^h 30^m found by Herschel refers to a portion beyond the line of separation.

The inner satellite of Mars must have a similar origin to that of the rings of Saturn, being far within the distance (6 radii of Mars) at which a satellite would perform its revolution in 24^h 37^m, the period of rotation of Mars. The circumstance that a satellite could be formed out of the nebulous trains under such conditions is accounted for by these being at 2·7 radii from the planet, or a little beyond 2·44, the limiting distance, within which a planetary body could not subsist, but would tend to dissolve into discrete particles. At a distance less than this limit a planet may have a ring but not a satellite.

In 'The Observatory,' No. 9, p. 280, Prof. Kirkwood has offered another explanation, according to which the present rapid rate of revolution of the inner satellite of Mars is the result of the contraction of the orbit, and consequent acceleration produced by the resistance of the less condensable portions of the nebulous planetary mass.

TRANSIT OF MERCURY, MAY 6.—In our last Number we gave some hints as to the phenomena to be observed, and we have only to add a few words as to the determination of the position-angle of the part of the sun's limb at which Mercury enters. This is a matter of great importance, and observers should take great care that they are watching the right part of the sun's limb. For those who have equatoreals provided with position-circle the matter is easy enough. The wire should be set tangentially to the sun's limb, or rather cutting off a small arc, at a point 45° from N. towards E., so that the reading of the position-circle vernier should be 45° from N. towards W. for direct image. For the benefit of those who have instruments with altazimuth mounting, we give, at the suggestion of a correspondent, a diagram showing the position of the point of first contact with respect to the vertex or upper point of the sun's disk. The angle from the vertex will vary somewhat for different places in Great Britain and Ireland. For Greenwich it is

35°, for Bristol 34°, for Edinburgh and Dublin 30°, measured in each case from the upper point of the sun's disk towards the east for *direct* image. From these data it will be easy to find the approximate angle for any other place. In the diagram, N is the true north point of the sun, V the vertex at Greenwich, and V′ the vertex at Edinburgh or Dublin at the time of first contact. The dotted line shows the path of Mercury across the sun's disk.



It may be well to mention that for all places in the British Isles, the first contact may be taken as occurring at practically the same instant of *Greenwich* mean time, $3^h\ 11^m$, as the variation arising from the effect of parallax would only amount to a few seconds. But of course if local time be used, the longitude (in time) must be added to $3^h\ 11^m$ for places east of Greenwich, and subtracted for places west.

FRENCH TRANSIT OF VENUS PHOTOGRAPHS.—M. D'Abbadie writes as follows:—"M. Cornu handed to me lately the first instalment of the printed measures made on the French Transit of Venus photographs, or rather Daguerreotypes. These measures are exact to ·001, and in some cases to ·0001 of a millimetre. The published copy gives only a minute description of the instrument used and of the manner of measuring. We have purposely deferred all calculations and discussion of results until all the measures shall have been published. Le Verrier told me that the sun's parallax was 8″·9, and Cornu's velocity of light gives the same result only when used with Bradley's constant of aberration. Struve's constant, now preferred by most astronomers, is open to several objections. The best constant would probably be obtained from Jupiter's satellites, and Cornu has proposed a new method for so doing."

THE COMING SOLAR ECLIPSE.—We learn from 'Nature' that great facilities will be given for observing the coming total Solar eclipse on July 29 from the neighbourhood of the Rocky Mountains. Messrs. Ismay, Imray, and Co., the owners of the 'White Star' line, have announced that they will take properly certified observers out to America and bring them home again for the sum of £20, which is rather less than first-class single fare; and the Pennsylvania railways company on their part have made arrangements for a round trip, at reduced fares, from New York, Philadelphia, Baltimore, or Washington, to Denver, which will form a convenient headquarters for intending observers. The return tickets for the journey to Denver and back (about 4000 miles in all) are:—from New York 73 dollars; from Philadelphia 71 dollars; from Baltimore or Washington 62·50 dollars. Thus observers will be carried from England to Denver or the Rocky Mountains and back again for the sum of £34. Intending observers should inform the Superintendent of the U.S. Naval Observatory, Washington, by letter, asking that a letter certifying to their identity be forwarded to their Consul at the port they may select for arrival in the United States.. This letter must upon delivery be countersigned by the Consul of the applicant to prevent mistakes; and on exhibiting it to the collector of customs, their instruments &c. will be delivered to them free of duty. The same letter must be presented to the agent of the Pennsylvania Railway Company, in order to obtain the advantage of the round-trip ticket.

IMPROVEMENTS IN SPECTROSCOPE, &c.—*Apropos* of some recent papers read by Mr. Erck before the R. A. S., Mr. Leonard Waldo writes from Harvard College Observatory that the idea of having a small finding-circle at the eye-end of an equatoreal was used by Prof. Wilson certainly seven years ago, and that in the Cambridge refractor they have the improvement of having two verniers at right angles [nearly], to read declination on both sides of the pier. Further that Alvan Clark and Sons have always made their solar spectroscopes to focus both collimator and observing-telescope with the same screw.

We may remark, however, that Mr. Erck's plan appears to be somewhat different from the ordinary setting-circle, as he takes advantage of the construction of the Newtonian reflector to utilize the position-circle as a setting-circle, its plane being, for this form, parallel to the axis of the telescope.

FOUR NEW MINOR PLANETS have been discovered since our last notice, viz.:—one of the eleventh magnitude on Feb. 28 by Palisa at Pola; one of the tenth magnitude on March 1 by Peters at Clinton; one of the eleventh magnitude by Prosper-Henry at Paris; and the fourth of the tenth magnitude by Coggia at Marseilles. These small planets are Nos. 184, 185, 186, and 187 of the group.

M. TISSERAND has been elected Member of the section of Astronomy in the French Academy as successor to M. Le Verrier, and will give up the charge of the Toulouse Observatory, in which M. Baillot is to succeed him.

A NEW astronomical Observatory is to be built at Bordeaux, on the hill north of the river.

EDITOR.

Books received:—J. L. E. Dreyer, *A Supplement to Sir J. Herschel's General Catalogue of Nebulæ and Clusters of Stars* (Trans. R. I. A. 1878)—Cincinnati Observatory, *Micrometrical Measurements of Double Stars*, 1877—O. Lohse, *Ueber Schmidt's Neuen Stern im Schwan*, 1878—Dr. Foerster, *Zur Theorie des Durchgangs-Instrumentes* (Berliner Jahrbuch, 1880), 1878—E. C. Taintor, *Catalogue of* 384 *Transit Stars* (Shanghai, 1878).

Astronomical Memoranda for 1878, *May* 15–*June* 14.

May.	G. M. T.	Phenomena.
	h m	
15. W.	14 8	♃ 4 Sat. Sh. I.
	23	♅ □ ☉
16. Th.	10 35	4 Scorp. Oc. R. 277°.
○	2 31	
	11 32	π Scorp. Oc.† 16°.
	18	☿ Aphelion.
19. S.	12 25	♃ rises.
	14 19	♄ rises.
	14 39	♀ rises.
	16 3	☉ rises.
	16 47·7	♃ tr. D. 18° 55′ S.
	20 9·6	♈ transits.
	20 13·7	♄ tr. D. 1° 44′ S.
	21 3·0	♀ tr. D. 3 52 N.
	22 46·3	☿ tr. D. 12 6 N.
20. M.	23 56·3	☉ tr. D. 20 1 N.
	5 59·0	♅ tr. D. 13 45 N.
	7 49	☉ sets. No real night.
21. Tu.	11	♃ ☌ ☾ ♃ 0° 24′ N.
22. W.	14 34	42 Capr. Oc.† 334°.
	14 49	♃ 3 Sat. Sh. E.
	15 17	44 Capr. Oc. R. 179°.
23. Th.	14 45·8	♃ 1 Sat. Ec. D.
☾	13 42	
24. F.	14 0	♃ 4 Sat. Oc. R.
	14 19	♃ 1 Sat. Sh. E.
	21 3·4	♀ tr. D. 5° 43′ N.
	22 31·9	☿ tr. D. 12 2 N.
25. S.	9	☾ Apogee.
	12	♃ stationary.
26. S.	14 7	51 Pisc. Oc. R. 258°.
29. W.	11 46	♃ rises.
	14 23	♀ rises.
	15 52	☉ rises.
	16 8·5	♃ tr. D. 18° 57′ S.
	19 29·3	♈ transits.
	21 4·2	♀ tr. D. 7 35 N.
	22 23·9	☿ tr. D. 12 50 N.
30. Th.	23 57·3	☉ tr. D. 21° 48′ N.
	0	♀ Aphelion.
	8 2	☉ sets. No real night.
31. F.	13 53	♃ 1 Sat. Sh. I.
●	13 48	
June.		
1. S.	14 33	♃ 1 Sat. Oc. R.
2. S.	7	☿ elong. 24° 2′ W.
	13 16	♃ 3 Sat. Oc. R.
3. M.	9 59	Mars Oc. D. 141°.
	10 39	Mars Oc. R. 251°.
	21 5·4	♀ tr. D. 9° 27′ N.
	22 21·8	☿ tr. D. 14 21 N.
5. W.	8 54	π^2 Canc. Oc. D. 136°.
7. F.		
☽	15 55	
8. S.	11 7	♃ rises.
	13 2·4	♃ 1 Sat. Ec. D.
	13 3	♄ rises.
	14 5	♀ rises.
	15 46	☉ rises.
	15 28·0	♃ tr. D. 19° 4′ S.
	18 50·0	♈ transits.
	19 0·3	♄ tr. D. 1 15 S.
	21 7·0	♀ tr. D. 11 17 N.
	22 25·3	☿ tr. D. 16 23 N.
9. S.	23 58·9	☉ tr. D. 22 57 N.
	8 12	☉ sets. No real night.
	11	☾ Perigee.
	12 34	♃ 1 Sat. Sh. E.
	12 40·8	♃ 3 Sat. Ec. R.
	13 13	♃ 3 Sat. Oc. D.
	13 34	♃ 1 Sat. Tr. E.
10. M.	7 39	75 Virg. Oc.† 32°.
13. Th.	21 9·0	♀ tr. D. 13° 3′ N.
	22 34·4	☿ tr. D. 18 40 N.
14. F.		
○	11 51	

† A near approach.

For the occultations of stars the position-angles of the points of disappearance or reappearance are given after the letters R or D. The angles are reckoned round the Moon's limb from *n* (true) in the direction *n*, *f*, *s*, *p* (*i. e.* from below towards the right hand, as seen in an inverting telescope). Stars from the British Association Catalogue are, for economy of space, denoted by their No. simply, the letters B.A.C. being omitted. **Editor.**

Meteor Radiants active in May.

No periodical showers of special intensity are due in May, but there are many radiants of some activity occurring this month. Amongst these may be mentioned the Coronaids, Cygnids, Draconids, Polarids, and Ursids, also a shower from near α and β–γ Lyræ, all of which appear to be long-continued systems of annual recurrence, though without any transient condensation. The results of each night's observations of these or other showers should in future be kept separate, and not grouped into long periods as hitherto. In cases where two or three fine nights occur consecutively there will, however, be no harm in incorporating the results. It is very desirable to ascertain whether the alleged long duration and fixity of many shower-centres are real or apparent, and whether meteors having precisely the same features continue to fall from each throughout their supposed activity.

The radiant-lists for April and June ('Observatory,' vol. i. pp. 417 and 61) should be referred to with the following list for many of the best observed and probably true radiant-points in May.

Mr. Greg's Cat. 1876.	Duration of shower.	Position of radiant.		Approximate star.	Authority.	Notes.
		R.A. °	Dec. °			
63.	Apr. 29–June 12	123	+40	31 Lynxis.	G. & H.	Denza, 120°, +40°, Apr. 3–14, 1869. A long-continued shower.
59*a*.	May	155	+ 9	ρ Leonis.	Schmidt.	Not seen by other observers.
...	May 3–15	170	−10	ε Crateris.	Denning.	A new shower deduced from the Italian observations, 1872.
...	May 16–31	175	+64	α Ursæ Maj.	Heis.	? Also at 163°, +58°, May 9 (Italian obs. 1872).
62.	May	202	+ 9	σ Virginis.	Neuma'r.	202°, +7°, May 3–15, 1872, Italian observations.
53.	May	223	−12	α Libræ.	Schmidt.	G. & H. 223°, −10°, May.
71.	May 3	233	+47	Quadrans.	Maggi.	Greg, 235°, +45°, May 16–June 2.
	May 26	237	+59	θ Draconis.	S. & Z.	Distinct to preceding.
69.	May 18–June 14	273	+34	α Lyræ.	S. & Z.	Mean of 3 showers, a well-marked radiant.
64.	May 25	280	+54	ο Draconis.	S. & Z.	*Draconids.*
66.	May 2	285	+12	ζ Aquilæ.	Tupman.	Heis, 292°, +15°, June.
	May 2	298	+ 5	α Aquilæ.	Tupman.	These are probably identical showers.
79*a*.	May 15	304	+ 7	ε Delphini.	Denning.	
70.	May 22–24	301	+37	γ Cygni.	S. & Z.	Also on June 19 at 300°, +41°, S. & Z.
...	May 15	320	+18	ε Pegasi. ζ Cygni.	Denning.	A new A.M. shower.
68.	May & June ...	329	+48	Lacerta.	G. & H.	Heis, 325°, +55°, May.
...	May 20–29	355	+42	κ Andromedæ.	Heis.	Denning, 354°, +41°, May 15–17, 1877.

The abbreviations in column 5 are:—G. & H., Greg and Herschel; S. & Z., Schiaparelli and Zezioli.

W. F. DENNING.

Position of the Sun's North Pole.

1878.	Pos.-angle.	Distance.
	° ′	° ′
May 20	340 30	91 50
25	342 8	91 15
30	343 54	90 39
June 4	345 48	90 2
9	347 49	89 26
14	349 55	88 50

The Moon.

1878.	h m	Geoc. Libr. at midnight. long. °	lat. °	1878.	h m	Geoc. Libr. at midnight long. °	lat. °
May 17...	rises.	+5·52	+5·76	June 1...	New.	−4·89	−5·27
18...	10 52	6·06	4·86	2...	sets.	4·74	4·21
19...	11 37	6·17	3·71	3...	10 49	4·42	2·90
20...	12 8	5·85	2·41	4...	11 19	3·95	−1·41
21...	12 29	5·16	+1·02	5...	11 41	3·34	+0·18
22...	12 46	4·15	−0·39	6...	11 55	2·61	1·78
23...	13 0	2·93	1·75	7...	12 11	1·74	3·33
24...	13 12	1·59	3·02	8...	12 25	−0·76	4·60
25...	13 21	+0·22	4·16	9...	12 40	+0·33	5·64
26...	13 34	−1·09	5·13	10...	12 57	1·47	6·33
27...	13 45	2·28	5·87	11...	13 19	2·60	6·62
28...	14 0	3·27	6·37	12...	13 47	3·64	6·51
29...	14 21	4·04	6·57	13...	14 29	4·49	6·01
30...	14 49	4·56	6·46	14...	Full.	5·08	5·16
31...	...	−4·84	−6·03	15...	...	+5·35	+4·05

A. M.

The Planets.

Mercury transits across the sun's disk on May 6 (see last No.), after which he is a morning star, attaining his greatest elongation west (24°) on June 2, when he rises about 45ᵐ before the sun.

Venus is a morning star. Her disk is gibbous, the illuminated portion on May 15 being 0·561, and on June 15 0·689, and the corresponding diameters 20″·6 and 15″·9.

Mars is an evening star, in Gemini.

Jupiter is a morning star, nearly stationary in Capricornus. His diameter on May 15 is 39″·2, and on June 15 42″·7.

Saturn is a morning star, nearly stationary about midway between β Ceti and γ Pegasi. The following are the dimensions of the ring:—

	Outer Ring.		Inner Ring.		Ball.
	Maj. Axis.	Min. Axis.	Maj. Axis.	Min. Axis.	Diam.
May 25	37″·56	2″·75	24″·98	1″·83	15″·1
June 14	38″·78	3″·19	25″·79	2″·12	15″·6

The south side is visible, the elevation of the Earth above the plane of the rings being 4° 12′ and 4° 43′ S. on May 25 and June 14, and of the Sun 1° 36′ and 1° 54′ S. EDITOR.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 14. JUNE 1. 1878.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 10th May, 1878.

Lord Lindsay, *President*, in the Chair.

Secretaries: Mr. J. W. L. Glaisher, F.R.S., and
Mr. A. Cowper Ranyard.

Mr. Glaisher announced 64 presents, including a set of 15 volumes presented by Dr. Radford, who had seen in the Annual Report a notice stating that donations to the library would be gladly received; also a set of his own works presented by M. Flammarion. The thanks of the Society were voted to the donors.

Mr. G. W. Hill, of Washington, Prof. Schönfeld, of the University of Bonn, Baron Dembowski, and Dr. C. Brühns, of the Observatory of Leipzig, were recommended as proper persons to become Associates of the Society.

Mr. Ranyard read a paper by Mr. Waterston on the Heat of the Stars.

The Astronomer Royal. With reference to the observations of the Transit of Mercury on the 6th of May and of preceding transits, I wish particularly to observe that there have been seen one or more bright points on the disk of Mercury during the transit; but, in my belief, these things are to be looked upon with great suspicion. And I have been led to this conclusion by observations which I have made myself of various kinds, bearing some relation to this, and which led me strongly to look, in the first instance, at such things as reflexion from the various surfaces of the lenses of which the eyepiece of the telescope is composed, reflexion from the cornea of the eye, and reflexions from the tubes of the eyepieces. I will mention to you a matter which occurred to me a great many years ago. In a room at the Observatory, where I was accompanied by my friends, scarcely known to many members of the Society at present, Mr. Francis Baily and Admiral Smyth, who were both very accurate men, we were looking out of a western window in the afternoon of a very fine day, admiring the prospect, when Admiral Smyth broke out at once with, "Why, there is a double star." Mr. Francis Baily looked immediately at the same place, and he said, "Yes; there is a double star;" and they described the places coincidently. I looked at the place, and there I saw a double star; of its visibility there was no doubt. I do not know whether they were so well satisfied with the appearance as I was; but I thought it incumbent on me to look carefully at this double star, and after a little time I found out what this appearance really was. The origin of this double star was a bright spot in the corner of the eye illuminated by the sun at the time, and the window through which we were looking was a plate-glass window, of which, as usual, the two surfaces were not exactly parallel; and the one gave me a reflexion which produced one of the component stars, and the other surface, in like manner, produced the other component star; and so the double star was produced. Since that time, when the controversy, as I may call it, concerning the origin of the prominences in a Solar eclipse was raging, an astronomer came to me in a great hurry and said he had found the prominences, and he could exhibit them at any time, and he exhibited them accordingly. I saw them, and examined them further, and scrutinized them a little, and I found that they were entirely caused by reflexions from the interior of the eyepiece tubes. On a third occasion an astronomer reported to me that he had discovered that one of the exterior planets was accompanied by a luminous atmosphere. I warned him at once to be a little cautious about it, and to examine every thing very carefully; but, however, I did not think that he was quite persuaded by my cautions; and at last I took the matter in my own hands, and examined the appearance which he described, and I examined it rather more critically, that is, I drew the eyepieces out of the tube of the telescope, and I put in black paper to line the tube of the telescope and various things of that sort, and found that most indisputably it resulted from reflexions in the interior of the tube. After these experiences I need not say that I distrust every observation which speaks of a bright spot on the surface of Mercury in the late transit. I need scarcely add to that that it is exceedingly improbable that in those various successive transits of Mercury at distant times, which, so far as we know, have no relation whatever to the time of rotation, the same kind of spots should recur. And altogether I think when the Society receives accounts of this kind, in which there are recorded bright spots seen on the surface of Mercury during the transit, I may invite them to regard those accounts with a little caution, to put their own interpretation upon them, and to ask if, amid the infinity of reflexions which pass between the various surfaces of the various lenses of the eyepiece of the telescope and of the eye itself, there may not be ample room for explaining these bright spots without any physical explanation whatever.

Mr. Christie. After the remarks which the Astronomer Royal has made I must be very cautious indeed in bringing forward the observations which I was enabled to make; but it must be understood that those were made in a limited time through intervals of cloud, and therefore I simply recorded what I saw, without giving any causes or trying to explain the thing. I may begin by saying that I saw a very minute star-like point in the centre of the disk of Mercury, which, so far as I could make out, did not depend on the position of my eye, though I had not sufficient opportunity to test this point thoroughly. I also saw round the planet a ring of light or halo of somewhat irregular shape, and of about half the radius of Mercury in breadth. This may have been an effect of contrast. Within this was an inner ring of perhaps a second in breadth, which seemed perfectly circular. I used both positive and negative eyepieces arranged in five different ways, viz.—with and without a Solar diagonal and Barlow lens, and also with the forms of polarizing Solar eyepieces described at a former meeting, in which the Sun's light was reduced to extreme faintness by three reflexions from glass surfaces.

Capt. Tupman has also given an account of the transit, in which he saw something a little different. He says, "I scrutinized the planet most diligently with powers of 110, 140, and 240, and saw no trace of a permanent white spot or of a ring, either bright or shadowy, surrounding the planet. Occasionally there was a whitish hazy patch on the planet; but it was not steady in position, being sometimes on one side, sometimes on the other, and once it appeared on both sides at the same time."

That, of course, is directly opposed to my observation—that is to say, Capt. Tupman did not see what I did. There were a number of other observers at Greenwich, altogether 14 observers, equipped with telescopes, and some of them saw a bright spot, others did not; some saw a bright ring, others did not—so that there was a charming difference of opinion.

Mr. Rutherfurd. I can only say that from my experience, and from the internal evidence afforded by the observations, I should think that the Astronomer Royal is entirely right. If one gentleman at Greenwich sees a dark ring, and another a light ring—if one of them sees a bright spot, and another one sees no spot—it seems to me that the explanation is to be found in the instrument; and it is quite possible, as the Astronomer Royal has well said, that such things may be seen among all the reflexions which take place from such a brilliant object as the Sun. I may remark that the image of the Sun made by a small lens, such as the lenses of the eyepiece, is a very small point. I have myself used an image of the Sun made by a small lens as a star in testing the figure of microscopic objects; and for that purpose it makes a very excellent star, very brilliant, and very much concentrated. The whole of the surrounding Sun around the image of Mercury would contribute to make this little star, and any little idiosyncracy in the inclination of the tubes which hold those lenses would determine its place upon that disk.

Mr. Dunkin. The papers which I hold in my hand, on which

Mr. Christie has already made some remarks, are those which were delivered to the Astronomer Royal after the transit. My account was written down before I knew what any other observer saw, and is thus an independent record. When first seen by me, Mercury was still partly obscured by clouds, and had advanced some distance from the sun's limb. The clouds gradually cleared away for a few minutes, when the images of the Sun and planet were superbly defined, and I may say that I have never in all my long experience observed the Sun with greater pleasure. In fact I can scarcely remember seeing a better definition of the granular surface. The observations were made with a 6-inch equatoreal by Simms, which had been used in the Transit-of-Venus observations. Mercury was intensely black, except that I occasionally saw a minute point of light near the centre, a little towards the following limb of the planet. There could, I think, be no doubt of the reality of this phenomenon. I also saw, during the few minutes of superb definition, a ring or corona of light around Mercury, its breadth being about one third that of the planet. This corona was, to my mind, of a half-tint, and was certainly not so bright as the Sun. It was perfectly uniform in breadth round the planet, and had a radiating appearance as in the solar corona.

Mr. Proctor. I happened to have my daughter with me, aged 16, who had not heard anything about this bright point in the centre; and when she saw the transit, her remark was at first that I had been playing a trick upon her, that I had put a piece of card on the eyepiece, and that she could see the centre where the compass-point had pierced it. That only shows that we saw the point, and that it was central. Then I noticed that as the clouds came over, the spot grew fainter. I think if it were a real phenomenon it ought to have grown brighter; but instead of that as the cloud passed over, it became fainter. That, I think, corresponds with the Astronomer Royal's opinion that it was due to reflexion in the eyepiece. That is all I noticed about the bright point, except that when I changed the power from a large field Kellner eyepiece to one of Browning's achromatic eyepieces, power C, I could not see the bright point. I could detect a faint mass of light in the middle, but I did not recognize the bright point, and then the light disappeared. There is one point which I feel doubtful about, and that is, that when the spot was fading out of view it seemed to assume a triangular form; but this appearance was perfectly momentary, and I feel so doubtful about it that I do not like to speak about it as if it were a real phenomenon. With regard to the ring I do not think that that is merely a subjective phenomenon. I think it is real. I could not see it on the side furthest from the limb when I directed my attention to that side, but the moment I directed my attention to the other side it was distinctly seen; and the moment I caught it it seemed complete all round. It was quite an optical illusion that I could see it all round. It seemed a mere thread of light outside a dusky ring.

Mr. G. F. Chambers. Perhaps, in view of the unfavourable weather which prevailed generally in England, on May 6, during the Transit of Mercury, observations of no great intrinsic importance may be worthy of record. It is on this ground only that I trouble the Society with the following remarks. About half an hour before sunset the Sun appeared through the clouds for some 10 minutes, and I was enabled to get a few glimpses of the planet. I could detect no indications of a spot, but a ring was visible round the planet. The breadth of it was, as nearly as I could make out, slightly more than the diameter of the planet; but I had no chance of making any measures. My wife, whose vision is naturally very acute, described it as a dark ring; but I am unable to feel quite sure whether it ought to be described as light or dark. I should say it was the former; but the hindrance caused by the fragments of clouds interrupting the view every few seconds, debarred me from an attentive inspection of the details. I was much struck with the blackness of the planet, and its obviously detached position in the heavens. The telescope I used was a 4-inch Cooke refractor, with a power of 60 on a diagonal eyepiece.

Mr. Brett read a paper on the same subject. He saw a ring round Mercury which he estimated to be twice as bright as the Sun; and his assistant observer, who has unusually keen sight, confirmed this observation, as did also Mr. Walter Pye. The last-named observer remarked that when the planet was first seen near the limb, the ring was excentric. No bright spot was seen, the power being probably too low (50). Two Cooke refractors of 4 and 3 inches respectively were used. Mr. Brett suggested that the bright ring might be explained by supposing Mercury to have a burnished surface covered with a dense transparent envelope.

The Astronomer Royal. Has an accurate examination been made as to whether the internal portions of the tubes of those eyepieces are dead black or not? I have the utmost suspicion of every thing where there can be a reflexion from the internal parts of the tubes.

Mr. Brett. I can positively answer that they were most carefully blackened.

Mr. Ranyard. Even with the most carefully blackened tube at a very oblique angle there is a very clear reflexion; as the angle increases you first see a red reflexion.

The Astronomer Royal. I know very well the effect of blackening tubes. In one case that came before me it has been to crush the inferences which had been drawn from the bright tube.

Mr. Sydney Waters. May I ask whether those bright spots have ever been observed on any of the dark spots which from time to time appear on the Sun? It seems to me that if it is merely an optical illusion these things should in some form be observed there.

The President. I think it would be advisable if we deferred the discussion of the papers until the subject has been exhausted: we have still two or three more papers.

Capt. Noble. I have a paper by Baron von Ertborn on his observations of the Transit of Mercury. Internal contact was observed by M. de Boë at $3^h\ 14^m\ 14^s{\cdot}9$ G.M.T., and by M. von Ertbon 1 second later, and a bright aureole and a small excentric spot were seen by several observers.

If that spot had any objective existence on Mercury, it is preposterous to suppose that parallax would shift it from one position in England to another position in Belgium. That seems to me absolutely conclusive proof of what Sir George Airy and Mr. Rutherfurd have said to-night, that it is an optical phenomenon, and one that has no objective existence whatever.

The Astronomer Royal. May I ask Capt. Noble whether ladies were prepared, by a model or by instruction of any kind, to be acute observers of the phenomena.

Capt. Noble. No ladies observed in Antwerp. It was Mr. Proctor who had a lady with him, and Mr. Chambers who had a lady's assistance. The Belgians were all men.

The Astronomer Royal. I would only add that if ladies have been properly instructed I have great confidence in their observations.

Mr. Penrose read a paper on the Transit of Mercury as seen at Wimbledon. He observed external contact at $3^h\ 11^m\ 34^s$ and internal contact at $3^h\ 14^m\ 35^s{\cdot}5$ G.M.T. At the latter, two narrow threads of light streamed outside the planet, and almost immediately after the Sun's limb seemed complete. Circular contact, with the planet clinging to the Sun's limb, was noted 4^s earlier. No *aureole* was noticed, but a *flickering* spot of light somewhat excentric on the side towards the Sun's centre, was seen. It seemed to behave like the spots which dance about in the vicinity of the brighter stars.

Mr. Ranyard read a paper from Mr. Cole on his observations of the Transit of Mercury as seen at Sutton.

Mr. Glaisher read a paper by Prof. Pritchard giving an account of observations of the Transit made at the Oxford Observatory.

Mr. Ranyard. We have another paper by Mr. Prince; but as he is not here I will give a short account of my own observations, which will not occupy many minutes. I observed the Transit at Dun Echt with Lord Lindsay's 15-inch refractor, which was stopped down to 10 inches. As the planet came up to external contact the sky was covered with clouds, which at no time were sufficiently opaque to prevent the Sun's limb being distinctly seen. From about ten minutes before the time of predicted contact I observed the spectrum of the sky outside the Sun's limb in the direction in which the planet was approaching. The spectroscope with which I was observing was placed upon one of the finders of the larger instrument, the viewing-telescope of the spectroscope being turned so that the red end of the spectrum below C was in the field of view. The slit of the spectroscope, which subtended an arc of $5'\ 45''$ upon the Sun, was swept outward across the path of the planet, but no variation in the brightness of the spectrum corresponding to the place of the planet could be detected. My reason for attempting this observation was that M. Janssen, in observing the Transit of Venus, thought that by making use of a screen of blue glass he had been able to detect the body of the planet projected upon the background of the corona some time before it was visible without the screen. His idea was that the spectrum of the corona was rich in blue rays. He does not give the ground for this assumption; and the observations, as far as I am aware, point the other way. Stone, observing the spectrum of the corona during the eclipse of 1874, was of opinion that the continuous spectrum of the lower part of the corona was rich in red light. And the polariscopic observations also seem to point to the same conclusion. I therefore thought it better to examine the red end of the spectrum; but I could detect no variation in the brightness indicating the position of the planet. It must, however, be remembered that these observations were made through passing cloud. I continued my observations at the spectroscope until some time after the signal for first contact had been given; but nothing indicating the position of the outer limb of the planet could be detected. I then passed to the larger instrument, on which was a Merz polarizing eyepiece, with a power of about 400. The planet had now entered quite one third of its diameter upon the Sun's disk, but I was unable to see any trace of the outer limb of the planet. I then passed back again to the spectroscope and watched until the planet was two thirds upon the Sun's disk, but saw nothing to indicate the position of the outer limb. On turning again to the large instrument with the Merz eyepiece only a small arc of the planet remained off the Sun's disk. The definition had now greatly improved; but I could not see any trace of the part of the planet off the Sun, and I certainly could not see any line of light round that part of the planet as described by the Transit of Venus observers. I watched the planet with the Merz eyepiece, turning the field alternately brighter and darker until the planet was entirely upon the Sun's disk. I gave the signal for internal contact at the moment when I estimated that the two disks (*i. e.* the disk of the planet and the disk of the Sun) were in geometrical contact. Within a second, certainly not more than two seconds, after this signal I saw a bridge of light separating the planet from the Sun's limb. I saw no ligament and nothing that I could describe as a black drop. When Mercury was upon the Sun's disk I carefully looked for rings round the planet and for the spot of light upon the disk, but could detect no trace of either the one or the other phenomenon; but with a cardboard diaphragm of less than an inch in diameter upon the instrument used by Dr. Copeland I thought that I saw a greyish ring round the planet, but did not feel quite certain as to its existence.

Mr. Common. Perhaps before the discussion takes place I may state what I saw. I was only favoured with a few minutes observation, at 3^h 20^m to 3^h 25^m G.M.T., and I had to use the whole of an 18-in. reflector for the purpose of seeing the planet, and I then got about three observations, and saw a large hazy white spot towards the south of the centre, and I strongly suspected between the planet and the limb a very minute dark spot. I was unable to continue the observation owing to the cloud. I put on the photographic apparatus for the purpose of seeing whether the white spot was projected on the screen. I got a view for a few moments, but I could not see any trace of it. The image of the sun was enlarged to about 18 in., and the planet Mercury appeared a little over $\frac{1}{8}$ inch in diameter; but there was no sign of a spot then; but it is worthy of notice that this very minute spot, about a diameter and a half from and between the planet and the limb; was distinctly perceptible. For the moment I saw it, using a Dawes' solar eyepiece and small aperture; possibly it was an illusion.

The President. I may say in a few words what I saw myself. Towards the beginning of the time of transit I was using the spectroscope on a 6-in. equatoreal, and I had arranged it after the manner which was employed a good deal in Italy by Father Secchi. I had a direct-vision prism within the focus of the telescope; the light passing through this cast a dispersed image of the Sun upon the slit of a spectroscope. I proposed to observe the exterior contact with a power of about ten prisms. However, I found as the time advanced that the clouds were getting so much thicker that I was obliged to keep on reducing the power of my spectroscope until at last I got down to two prisms and the lowest power eyepiece, and I noted that by the time of my chronometer, $5^h\ 3^m$, the limb was scarcely visible. However, in the field of view as I had arranged it at a certain time, I saw a faint continuation of the reversed C line suddenly commence to get shorter, which gave me warning that it was the planet coming down upon the chromosphere. I saw it getting shorter and shorter for 14 seconds, and I was enabled to trace the C line being cut down by the body of the planet; I then saw in the spectroscope a perfectly clean and sharp definition of the first nick in the limb of the sun. That I noted as the first exterior contact. I watched this for a minute or two, and I became sensibly aware of the clouds clearing away, and the light being too great to bear with a small battery of prisms. I tried to get my spectroscope again into the proper condition to observe the internal contact; but unfortunately, while my eye was away, it was effected without my observing it. I had another careful look at 9^m, at 12^m, and at 16^m after contact (the time is not reduced). At 16^m I used the second-power eyepiece, and the planet was well on. I noted the planet as seen in the C line of the spectrum being sharp, clear, and clean. Using 10 prisms no sort of halo is visible, and Mercury is black on a bright ground. Using the C eyepiece the shading distinctly decreased from the centre of the planet outwards. I then took the spectroscope off the telescope and examined it with a C eyepiece and a full aperture of 6 in., and I noticed that the sun's image was remarkably good and the definition was very fine, and that Mercury was a perfectly clean disk, with an absolutely sharp outline and not a trace of any spot whatever. Also I can say there was no ring;

but I noticed that there was a distinct difference in the tint of darkness between the centre and the limb. I then endeavoured to obtain the rings round the planet, which I was enabled to do by contracting the opening by means of an Iris diaphragm. I may say that I looked through my own 6-in. telescope, a 15-in. telescope, and a 4-in. telescope, and I saw no trace of a spot or any ring, and none of my party saw any of them.

Mr. Ranyard. When Mercury was on the Sun I placed the slit across it, but was unable to see any alteration in the solar spectrum. It was very difficult indeed to bring it up at all tangentially to the planet's limb. An air-line went across, and it was altogether an observation in which no reliance could be placed.

Dr. Huggins. As my observations of the transit in 1868 have been referred to with regard to seeing a spot upon Mercury, I would wish to call attention to one point which I then very carefully observed. I watched the white spot upon Mercury and very carefully at the time of egress of the planet. At the moment when the planet passed about half off the sun this spot instantly vanished. This observation appears to show that the spot was an optical and not a physical phenomenon. For the late transit I had prepared an apparatus which would have enabled me on Monday to have taken photographs of the spectrum of the sun with Mercury within the field of the slit of the spectroscope. The photographs, embracing a large range of refrangibility, would have permitted any modification of the solar spectrum to be detected arising from the absorption of atmosphere around Mercury. Most unfortunately in my neighbourhood the sky was clouded nearly the whole of the time. There were two or three minutes in which the Sun became a little clearer, and I seized the opportunity to take a photograph; but from the thin cloud I could not be quite sure whether I had included Mercury within the slit. However, upon the photograph there is a black band the breadth of which corresponds exactly with what would have arisen from Mercury. I am not yet able positively to say whether I may regard this band as due to Mercury. If it be so, there is obviously no very considerable modification of the solar spectrum; but I am not quite prepared to say whether or not some very faint traces may or may not be detected quite close to the limb. I think it necessary to strengthen the photograph and to make some further careful comparisons before speaking positively on this point.

Mr. Christie. I omitted to state that the ring or halo was seen most distinctly when the field of view was limited by a small diaphragm to about three diameters of Mercury, and when the brightness was reduced almost to the vanishing point by means of a polarizing eyepiece. On trying an experiment afterwards with a black disk on a piece of paper, I found that I could see very much the same sort of halo round this black disk, and that that halo comes out more distinctly when a dark glass is used to reduce the light, so that it is quite possible that the outer halo which I saw was the effect of contrast.

Mr. Brett. Then the argument, according to you, is that the weaker the surface the more it would be affected by contrast, which seems to me a very strange doctrine.

Mr. Neison. As to what Mr. Christie says about being able to see a sort of halo around a dark object like Mercury, it is confirmed by his simple experiment. Looking at the dark circular disk on this piece of paper I see no halo around it; but if I look at it through this dark glass I distinctly see a sort of faint halo round the dark spot, and about equal in breadth to the radius of the spot. The experiment is worth trying.

The President. I notice that in the paper which Capt. Noble read he gives the time of contact as $14^{m}\ 14^{s}{\cdot}9$ Greenwich mean time. May I ask if the longitude is fairly accurate, because it is considerably before any other observation of internal contact that I have seen?

Capt. Noble. I believe that M. de Boë determined the longitude of his observatory with very considerable accuracy indeed. But I must expressly disclaim any responsibility beyond simply reading the communication, which has been sent to me from men who made the observations.

Admiral Sir E. Ommanney read an extract from a newspaper giving an account of the observation made by M. de Boë.

Mr. Rutherfurd. Gentlemen, the matters which I have to bring before the Society are twofold. The little plates which I have with me are photographs or positive copies of three negatives of the sun taken in 1871, which I caused to be made a few days ago in New York on first seeing the announcement of M. Janssen that he had obtained photographs which showed the granulations on the Sun's surface, the rice-grains or whatever else you choose to call them. I had the pleasure of seeing one of M. Janssen's photographs in America; and while I bring these to the attention of the Society simply to claim that he was not exactly right in stating that they had never been before seen, I do not claim that my photographs are equal to those of M. Janssen. But in these photographs the granulations are quite easily to be seen. They were taken in August 1871; and I bring them up, not for one moment to diminish the credit due to M. Janssen, but simply to make a small correction in the history of that part of astronomy.

I have here another matter not cognate to that, viz. a photograph of the solar spectrum, or so much of it as I find can be taken with a high power, obtained with an interference-plate constructed by myself—that is, I constructed the machine which drew the lines, and you require a lens to see them. The interference-plate with which this photograph was made had about 17,300 lines to the inch; and the photograph was taken in the spectrum of the second order. [Mr. Rutherfurd here exhibited a magnificent photograph of the spectrum, which reached right across the room.] I came away from New York in a hurry, and I had not time to put the number of lines upon it. You can see line E and *b*, and F and G, and the two H lines, and the ultra violet part. I was very agreeably surprised, on putting the ruler upon it, to find the wave-lengths are almost a scale of equal parts, for there are many things to prevent their being so. This represents prints from 28 different negatives, which were first taken on a small scale, and from those, large positives were made, and then again enlarged negatives. Then in joining the paper there is room for all the error which I found; but I suppose that the error never amounts to more than a millimetre and a half.

Perhaps I may be permitted to say something of the spectroscope with which this was done. I found the greatest assistance and the greatest increase of power in doubling or trebling the focal length of my collimator. Everybody familiar with the spectroscope will know that with a slit of a given aperture, if you double the collimator, your slit is diminished one half; and consequently if you double your collimator you can open your slit twice as wide and get double the light. I found that a very great advantage. Then the form of condenser which I used was peculiar. It was simply the achromatic objective of a large-sized opera-glass of 2½ inches. The rays were received upon the convex lens, which rendered them parallel, so that I kept the image of the Sun on the objective of my collimator within as small dimensions as possible, to diminish the risks of the aberrations of the objectives and diminish the risks of falsity of my lines; and altogether I found that that was the best form of condenser which I could use. The interference-plate was drawn on speculum metal. It is not the best possible material; I hope some better will be found; but it is the best that I have been able to devise. The interference-plate drawn upon glass, and afterwards silvered, will give a much more brilliant light; but we have to deal with the two surfaces of the glass, and it is very difficult to make those two surfaces accurate; and if you get them accurate you are very apt in polishing them to get them wrong; and you have to polish them very highly in order to draw lines 17,000 or 18,000 to the inch. In the case of speculum metal it is very curious that those fine lines develop all the crystallization of the metal; but notwithstanding that, the definition is quite fine, and I have not been able to find a substitute which will produce a better effect.

Mr. Ranyard. If I understand you, the image of the Sun is thrown on the slit.

Mr. Rutherfurd. Yes; it is thrown directly on the slit of the collimator. This represents nearly the central portion of the hemisphere of the Sun. There is no evidence about the bright lines. At the place where Dr. Draper's oxygen lines should come in there were a great many lines, some bright and some dark, but it is difficult to say whether they were due to oxygen or not.

The President. I am sure that in expressing your thanks to Mr. Rutherfurd I shall only be echoing the feeling of the meeting for his most interesting communications and for the really magnificent work which he has done. I certainly have never seen any thing before so fine as this photograph.

Mr. Ranyard. May I ask if the photographs of the Sun were taken with a very short exposure indeed, and whether there was any special correction?

Mr. Rutherfurd. I suppose that in taking the photographs of the extreme part the time amounted to seven or eight minutes; whilst in the more strongly active portions in the neighbourhood of line G a quarter of a minute would be sufficient. These photographs of the Sun's disk were taken with the very smallest exposure that we could possibly get.

Mr. Christie. M. Tempel, of the Observatory of Florence, has sent me over some specimens of his drawings of nebulæ, which he thought might be interesting, remarking, in the letter which accompanied them, that the main object of his criticism is to show that drawings of nebulæ leave much to be desired before sure hypotheses can be built on them, whether in relation to their outward form or to their variability.

Up to the present time M. Tempel has made 160 drawings of nebulæ, and has so far discovered 50 new nebulæ; so that it would appear likely that there are a great many more of these objects in the heavens still to be picked up.

The President. In addition to Mr. Christie's remarks I may mention that I am pretty well acquainted with M. Tempel, and I can speak feelingly of the difficulties which he has had to contend with in his work. He has a very fine telescope of, I think, 11-in. aperture, made by Amici. This telescope is mounted equatoreally with circles, but the circles are not divided; consequently he has literally to fish for his nebulæ. I remember on one occasion he was desirous of showing me a special nebula, but he was unable to find it. I think, considering the beauty of the drawings, it shows what wonderful power he has, seeing that he is able to make these drawings and at the same time to move round a heavy telescope by hand without any clockwork to help him.

Mr. Watts exhibited a photograph of an observing chair, and read a paper explaining its working and its advantages.

Capt. Noble. I think it much to be desired that some mode should be discovered or adopted for drawing nebulæ as they are really seen in the heavens. These drawings of M. Tempel's are very beautiful, but they are very misleading in having them dark upon a light ground. If any ingenious gentleman with plenty of time upon his hands will find out a method of representing accurately the nebulæ as they appear, that is, light upon a dark ground, he would do a real service to astronomical science.

Mr. Christie. M. Tempel has directed his attention to that point at the suggestion of Mr. Brett; but there are very considerable difficulties in the way of it. He mentioned three methods. One is by engravings on steel; another by lithography, which has been applied with success by Mr. Trouvelot in the Harvard College publications; and the third by drawing on very fine paper and then reproducing by photography, so as to obtain a negative. That he has tried in some of the drawings published in the Milan Observations, but he is not altogether satisfied with any one of the three methods.

Mr. Proctor. They might be printed in white ink, or perhaps with a sort of greyish-white ink, with a black ground.

Mr. Ranyard. In trying to draw Coronæ I endeavoured to have them printed white, but I found no printer that would print on black paper. The difficulty is to get any thing sufficiently opaque for our purpose.

Mr. Howlett. There is one method of representing nebulæ or comets, and that is by scratching out the black pigment of black paper, and so leaving patterns with very delicate gradations.

The following papers were announced:—

S. W. Burnham. β Leonis and B. A. C. 3992.

C. L. Prince. The Transit of Mercury, May 6, 1878.

W. F. Denning. The April Lyrids and contemporary Meteor-showers.

S. W. Burnham. Note on the Duplicity of the Companion to Rigel.

The following were ballotted for and duly elected Fellows:—

Maxwell Hall, M.A., and James L. M^cCance.

The Interior of the Earth *.

Sir George Airy remarked that the nature of the subject was different from any upon which he ever lectured before, in regard to its indefiniteness and to the difficulty he should have if he considered it to be his duty to lead them definitely up to some point. He could only give them some idea of the theory to which he wished to lead them, and in doing so he would advert collaterally to a good many points which might be valuable. He proposed to divide his address into three parts. The first would relate to the measures of the earth; the second to observations on temperature; and the third to the manner in which they might suppose the earth to have been formed, especially with regard to the nebular hypothesis; and after that he would add some remarks on the conclusions to which these lead.

He described the process called triangulation, by which a large part of the contour of the globe is covered, and by which it is possible to lay down a map on which the distance between any one point and any other point is ascertained to within a few inches; how that this was valuable in ascertaining the dimensions and figure of the earth with the aid of the zenith sector, an instrument for measuring the apparent distances of stars from the point overhead; and how, by comparing the measure of distance on the ground with the change of the apparent distance of a star from the point overhead, the curvature of the ground was ascertained. He showed on a large globe the principal lines of measurement which had up to this time been made for this purpose. From these measurements, there was no doubt that the curvature of the ground is almost the same everywhere; that the earth is very nearly a sphere of 8000 miles in diameter, or 25,000 miles in circumference. When he spoke of the surface of the earth, it must be understood that he spoke of the sea-level. Above that level stand mountains, and below are the depths of the sea. But although these inequalities of surface are taken into consideration by those who go accurately into calculations, they are comparatively very small. Suppose he were to make a sphere twenty-five feet in diameter, representing the earth, how much did they think the mountains would rise above the level? One fifth of an inch. Well, of course, that never could be seen; and it was a thing that in all ordinary calculations might be neglected. So that they might say that the earth was a sphere, with an exception he would mention presently. Then there was another thing which was important to their present subject, and that was the density of the matter of which the earth is formed; and this was a matter which had engaged the best experimenters in two or three ways. The first of the experiments of this kind is a very celebrated one known as the Schehallien experiment, so called from its being an experiment on a mountain in the Scottish Highlands (Perthshire) particularly adapted to these measurements, which are most favourably carried on in the north and south direction. It was found that the mountain Schehallien disturbed the plumb-line, causing a deviation from the vertical of 11″ or 12″. Then if that mountain, whose dimensions we can measure, turns the plumb-line so far, what is the proportion of its attracting mass to the attracting mass of the earth? And as we know the size of the mountain and the size of the earth, we can compare the density of the mountain and that of the earth. This process was gone through with great care, and it was found that, taking the density of the mountain as we could trace it by its constituent rocks, the density of the earth would be about four and a half times that of water, or about twice the average density of the surface rocks. The earth had density everywhere, but was more dense towards the centre than the outside. The next experiment is known as the Cavendish experiment. Here was a very light rod of deal, six feet long, suspended by a fine copper or silver wire (which is the most delicate suspension we can have) forty inches long, within a wooden case to defend it from currents of air. At each end of the lever was hung a ball two inches in diameter, and by a simple contrivance a pair of leaden spheres, weighing together perhaps 300 lb., were brought simultaneously into the neighbourhood of the balls (but outside the case), on opposite sides, so that they might attract the small balls; and the experiment was varied until, by a series of calculations, the density of the earth was ascertained, and gave a greater result than before, namely, that the average density of the earth was about five and a half times that of water. Then the third experiment was one which he made himself in the Harton Colliery, near South Shields. That was by seeing how much the force of gravity was altered by going to a great depth, the force of gravity being ascertained and compared at the top and bottom by the swinging of a pendulum. From that a calculation was made, and it gave the density of the earth as six times that of water. He believed the best calculation was that founded upon the Cavendish experiment, and was quite willing to take something like five and a half times the density of water as the average density of the earth, including every part of it. There were consequences which followed from that which were certainly very striking. As this density was rather more than double that of the surface rocks, it showed that towards the centre the earth was more condensed than at the outside. But there was one result of the calculation which rather startled him when he made his own calculations on the subject. Since these rocks press upon each other more and more the further you go down, what is the pressure upon the square inch when you approach the centre of the earth? Many gentlemen there would have heard of a pressure of 50 lb. or 100 lb. on the square inch, and perhaps the greatest pressure we know is that by which tough Aberdeen granite is crushed—10,000 lb. to the square inch. But it must be 30,000,000 lb. to the square inch in the centre of the earth; and it is an astounding thing to imagine what consequences may follow. We have no idea of any such degree of pressure, and cannot therefore conceive what its consequences may be. Perhaps thereby gas may be squeezed into gold or platinum, and powder to solid, or solid to powder—we cannot tell what it does. That enormous pressure, and our total ignorance of it, is one of the difficulties and troubles of this case. He thought the general state of the earth would be understood from what he had said; and now he came to the rotation of the earth. The earth revolves, as everybody knows, in the course of a day; and everybody knows also, from the housemaid who whirls her mop to the greatest philosopher, that rotation would swell out the middle of the earth. Calculations have been made upon that, and the result is that the diameter of the earth in the equatorial direction is greater by about 1-300th part than the diameter in the polar direction. When they found that the measurement of the dimensions of the earth agreed so well with that conclusion, it led them to the further conclusion that the earth is, or has been, in a fluid state. In corroboration of this, he would mention a singular circumstance which occurred in our Indian Survey. In proceeding northward from Cape Comorin, the curvature of the earth agreed very well for many hundreds of miles with that found in other parts of the earth (with due reference to the elliptic form of the earth). On approaching the Himalaya Mountains the plumb-line was sensibly attracted by the mountains. The late Archdeacon Pratt investigated, from the form of the mountains and the density of the rocks, the disturbance of the plumb-line, and found that it ought to be much greater than it really is. Sir George explained this by supposing that the whole of that country is floating upon a dense fluid, and that the thick mass of the lighter mountain-matter sinks deep in the fluid, and that the displacement of denser matter neutralizes almost entirely the attraction of the lofty mountains. The form of the earth is not such as would be taken by a solid structure, but such as would be taken by a fluid mass with solids floating upon it.

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* Abstract of Address to the Cumberland Association for the Advancement of Literature and Science, by Sir George B. Airy, K.C.B., F.R.S., Astronomer-Royal. Revised by the author.

In the second part of his address, Sir George Airy referred to what is known about the temperatures. They knew something of the rate at which temperature travels through the earth. The experiments on this point had begun, as many good experiments have begun, with the French, who fixed thermometers with very long stalks to the depth of twenty-five feet in the ground. These experiments were followed up, after some time, with similar thermometers at the Observatory at Edinburgh, and about the same time at the Observatory at Greenwich, and there the deeper thermometers were read every day. The first and most conspicuous result of these experiments is the retardation of the seasons. At the depth of twenty-five feet, high midsummer heat occurs at December, which shows that it takes five months for the heat to travel down that depth. If you compute it further, it takes 100 years to travel a mile; so that if the crust of the earth is 100 miles thick, it will take 10,000 years for the transmission of heat through it. This showed that really, after all, we may have a great deal of heat below us, and that it will not come to us for a very long time. It will come at last, but it will come travelling up slowly, and in the meantime the radiation from the surface of the earth will carry it off very rapidly. So that it is quite possible that with a cool surface there may be a great deal of heat below. In every part of the earth there is evidence of intense heat in former times. The extent of volcanic action is partly lost on the earth by the effects of air and water; but when they looked at the old rocks, they found there had been volcanic action almost everywhere. In our limestone rocks, for instance, there are basaltic veins, which in some parts go by the name of toadstone, which are certainly the result of volcanic heat enough to produce fluidity. Almost everywhere they found that there were volcanic streams intermixing with all the rocks; and even although the surface of the earth had been free from volcanoes in a given district for a time, yet there had always been volcanic action very near, enough to force in veins of lava from time to time. It seems, therefore, that we are entitled to say that we have always been near a great deal of heat—probably we have been much nearer it than at the present time, but still we are near enough to experience a great deal even in these countries. Repeated experiments have been made on the increase of temperature as you go down in mines; and the conclusion has been come to that the temperature rises one degree Fahrenheit, sometimes in sixty and sometimes in 100 feet.

There is a mine in Cornwall in which he had walked in a stream of water at the bottom actually scalding to the legs; and everybody knows what quantities of water there are in the hot springs. And then there is the great display of the volcanoes, which come from a great deal of heat somewhere; and in places where volcanoes are extinct we can trace a sort of basaltic continent, so to speak, up to the very mouths of the craters from which the lava has come. So that there has been in all former ages undoubtedly much more heat than at present. There was another matter on which he would desire to speak, but with no great boldness, and that was the change in magnetism. The subject of terrestrial magentism is one of the most obscure in the world; nevertheless, looking at the direction in which it always is towards the colder parts, and tracing its general phenomena, it may be effected by thermo-electricity, and that may be produced by the constant wear going on in the interior of the earth, where the fluid lavas are consolidating themselves. Within a few years the voyage of the 'Challenger' has been made; and he had little hesitation in saying it was one of the most important in the scientific history of the world. In crossing the great seas they sounded to great depths, and measured in a satisfactory way the temperature of the water down to the depth of five miles. They always came to cold at the bottom; and there are great controversies whether the cold can come in deep sea streams from the Arctic and Antarctic frozen regions. He thought that had some influence; but he thought the bottom of the water and the ground at those great depths is cold: he did not think that part of the earth partakes of the same heat as other parts; that he only expressed as his opinion, in which, of course, he might be met by the disbelief of a great many persons. That was the state of things as we know it regarding the temperature of the earth—that, while there is reason to think that some parts of the surface are cold, there is evidence everywhere that there has been enormous heat almost all over the earth. Some parts of the crust of the earth under the deepest seas are still perforated by volcanic islands. In some places the heat comes very near the surface. That he looked upon as an important fact, leading them to a theory of what the state of the earth really is.

On entering upon a matter which was undoubtedly one of the boldest speculations in modern science, which was the formation of the Earth—he could not say its creation, but the way it got into its present shape—he had to premise that the theory on which he had to speak, which is known as the nebular hypothesis, is the conception of a very bold and vigorous intellect indeed. Laplace it was who remarked that all the planets and satellites revolved in the same direction round the Sun, and all of them turned on their axis in the same direction: and it was difficult to deny that there must be some general cause for this. It naturally occurred to Laplace that if we can find something that is contracting its dimensions, and which has a little rotation to begin with, then with every contraction of dimensions that rotation would become more rapid, till it might go to any degree, depending upon the condensation of its various parts not condensed before. Then can we look at any matter which is being thus condensed, and which might so form systems such as ours, with sun, planets, and satellites? There are a series of bodies in the sky which did not attract much attention in former days, mainly because telescopes were not so large, but which are now catalogued by thousands. These are the nebulæ. The name denotes their cloudy appearance. They are small bodies among the stars, sometimes appearing to have stars in them, or to be connected with stars, and sometimes not. They have the strangest and most capricious shapes imaginable. If a nebula is contracting its parts together so as to form a world, that rotation in the course of condensation will become so rapid that it may form suns and planets and earths around it; and on this supposition there is no difficulty in making a complete solar system out of such a mass as that of the nebula in Orion. Observations made lately by the largest telescopes—those of Lassell and Lord Rosse, both of which are remarkable telescopes of the largest class—have brought to light a number of nebulæ possessing a spiral appearance; and they seem to have some bearing on the supposition that the nebulæ are contracting and getting into a rotatory state. But these changes go on so slowly that they had not been able to answer with certainty for any of the changes of which he now spoke. The whole thing is theoretical, and yet, as it seemed to him, in the highest degree probable. Supposing this to be the case, these nebulæ would rotate, and in their compression would become very hot. There is no doubt that condensation would produce enormous heat, and it seems we have there sufficient explanation of the great heat we find below the surface of the earth and in other places. We suppose that the stars generally have been formed from the condensation of nebulæ; and there is a circumstance which was worth mentioning. A series of observations founded upon optical experiments has come to light within late years which has done more to reveal the secrets of nature than any thing before—this was by means of the spectroscope. By voltaic action sparks may be produced which derive their character—sparks like those of an electrical machine—in a great measure from the metals from which they spring. A spark springs from metal to metal, and the character of the metals gives different characters to the sparks. The sparks are viewed through a prism, which causes a line of light to appear like a coloured sheet of light, called a spectrum. We have one set of these spectra produced by iron, another by nickel, others even by hydrogen gas, and so on, and these are observed and catalogued with great care. When we come to observe the light in the stars in the same manner, we find there are scarcely two stars alike; some of them have the same spectra as that given from iron, and others have spectra from a number of different things; and we are actually able, by legitimate reasoning from this, to say from what the stars are made—what metals and other things they are made of; and, as a general thing, the stars are not all alike. So that in this nebular hypothesis we are not bound to say that the nebulæ are all of the same materials; and we conceive that by comparing the bodies which we know in the solar system with those of the stars, we may arrive at an idea of the variety of materials of which the planets are composed. We cannot find any thing different in comparing the light of the planets, because they all derive their light from the Sun, and they do not present any difference of appearance in the spectrum. But we can draw conclusions from their relative density. As he had said to them, the average density of the Earth is probably five and a half times that of water. They knew that the Sun is only once that of water. What the Sun is he could not tell, but it is a very poor light creature indeed. The density of Mercury is perhaps rather greater than that of the Earth. The density of Venus and also that of Mars are also much the same as that of the Earth. Then after that comes a shower of little planets, about 200 of which have been observed up to the present time, and he could not tell what they are made of. Then there are Jupiter and Saturn, which are no heavier than water. So that it appears clear that, assuming the formation of these things by the condensation of nebulæ, on the theory he had mentioned, the different parts of the nebulæ which have contributed to the solar system are very different. Well, that being considered as established, it follows that in the constitution of our Earth there may be parts of very different density. He should say that the high and prominent parts of the land are made of something light, and the heavy and dense parts are those covered by a considerable quantity of water, which have sunk deep into the central lava on which, he conceived, all things are resting.

And now he had come pretty nearly to the end of his theory, and he would show them what he feared they would call an absurd representation of what he conceived the state of the Earth to be. [The lecturer drew attention to a diagram of an "ideal Earth," roughly showing his theory—some parts of the crust of the earth being thick and coloured darkly to indicate density; some thick and not so dense, and all admitting of volcanic eruptions from the interior, which was represented as lava.] Remember that every thing here is exaggerated. It is not intended to be a correct representation. It is a caricature of the most extravagant kind; but if it conveyed to them the broad ideas that had impressed themselves upon his mind, it would be doing the right thing. He thought a large proportion of the centre of the Earth is fluid and hot, and he thought that upon this there were certain divers classes of something like solid matter. In all these parts there are cracks or chinks through which volcanoes burst out where the cover of the Earth is very thin. In some places you have two or three volcanoes together. There is one instance in Europe, where we have Etna, Stromboli, and Vesuvius. In this diagram he had condensed to the best of his conjectural power his supposition as to what the state of the Earth really is; and if any one chose to find fault with it he would not quarrel with him. He only gave it as a sort of inference from a number of things he had said.

On Changes in the Plumb-line.

Since Lalande published his treatise I am not aware that any work on astronomy has expressly stated that the plumb-line is motionless in its direction. Yet it is tacitly admitted to be so. Whenever observation shows a discrepancy in the latitude of a place or in the N.P.D. of a star, the whole difference is ascribed to faulty measure or to some unexplained instrumental error. Astronomers rely on *mean* results of angles referred to the zenith or nadir, and thus mask a phenomenon which is well worthy of close inquiry.

The simplest mode of settling the question seems to be attained by observing from a distance fixed spirit-levels. I did this in 1837 at Olinda (Brazil), in 1842 and 1843 at Gondar and Saga, in Ethiopia, and afterwards at Audaux in France. But levels are delicate only after a jerk; I therefore devised a much more sensitive apparatus. I call it a "Nadirane," and proceed to describe it.

A truncated cone of concrete, 10 metres high and with a hollow axis, was built on a rock at Abbadia near Hendaye. The top of that cone is 2 metres broad and forms inside a solid vault pierced by a vertical hole 12 centimetres wide. This is covered by a thick brass plate cemented into the concrete, and bearing two cross wires of platinum in an empty circle of 20 millimetres diameter. Through this circle and in a vertical direction a ray of light is sent downwards, by a small mirror, to the bottom of the narrow well at a distance of 10 metres. There the light goes through a heavily framed lens 12 centimetres wide, and is reflected back by a surface of quicksilver. As the lens has the platinum wires exactly in its 10-metre focus, and as it is so placed as not to be precisely horizontal, it reflects the image of those wires in their neighbourhood. Above them is a vertical microscope topped with a filar micrometer and a circle of position which can be moved round in any azimuth. When the lamp is lit and the mercury turned on fresh to ensure a clean reflecting surface, the microscope's field of view shows the cross wires and also their reflected image. It is thus evident that if the plane of mercury remains always parallel to its previous position, the distance of the wires from their image is not altered. If, otherwise, that distance is measured first in the meridian, then in the prime vertical, and, if necessary, in any other azimuth. On account of the mercury's great distance from the wires, the slightest deviation of its surface from its previous position is easily perceived and measured. In this apparatus it is assumed that the relative positions of the large lower lens and of the platinum wires remain unchanged.

From motives explained elsewhere, I use the decimal division of the right angle or quadrant, which is the natural unit for all trigonometrical functions. One division of my micrometer is equal to ·0000001 quadrant or 0″·03; but I use, as a more convenient unit, that of the sixth place, which our staff officers write 1″.

It seldom happens that unexpected results do not follow the opening of a new line of research. A few of these are worth mentioning.

Small earthquakes, unnoticed elsewhere, were often seen in my apparatus. Sometimes the image would disappear altogether and return, after several oscillations growing gradually smaller. Weak shocks leave the image in the field of view, its greatest excursion embracing an angle of 20″. My prettiest microscopical earthquake gave out the same cross-looped curve as that traced in Algiers by Cochard's pendulum under the disastrous shock of January 1867. The angle marked there was 18300″; mine was only 7″. It occurred in August 1869.

A stranger phenomenon is what I call *wriggling.* The image vibrates now and then like a high-tone tuning-fork, but on the same spot, then leaps very suddenly to a distance of 1″ or 2″, still vibrating, yet seemingly riveted for a while to that new spot.

These sudden changes prepare us for more important ones in the direction of the plumb-line. I subjoin a few of my results, giving the angular change of the vertical line in the meridian only, but premising that it varies in all directions:—

In	From	To		The nadir moved ″
1867,	Oct. 28	Dec. 1	....	8·6 to the north.
1868,	Nov. 21	Dec. 1	....	0·6 „
1869,	Oct. 12	Nov. 20	....	4·2 „
1871,	Oct. 10	Dec. 1	....	2·9 „
1872,	Jan. 30	Mar. 26	....	13·9 to the south.
1872,	Oct. 12	Nov. 26	....	0·5 to the north.
1872,	Oct. 10	Dec. 1	....	2·1 to the south.

I have registered more than 2000 observations of my "Nadirane." They prove, at least, that the vertical line is almost always changing within limits as yet undetermined.

On the 27th October, 1871, its position was altered as much as 7″·4 in the short space of six hours. The image of my cross wires is rarely altogether tranquil or motionless for a whole day. Until now it has been always disturbed when the sea is rough, showing thus that waves falling on the shore, which is 400 metres off, give a tremulous motion to the beach, even at that distance.

During his stay in Campbell Island for the Transit of Venus, Bouquet de la Grye, who was then unaware of my researches, made similar ones with a heavy pendulum which commanded a vertical tracing lever. He thus proved that small deviations of the vertical line exist also in the southern hemisphere.

It is therefore probable that small changes of level are going on everywhere. As to their causes, they can scarcely be ascribed to temperature, for the thermometer-curve does not agree with that of the vertical line. There remain two suppositions: (A) for some unexplained reason the centre of gravity of our globe is in motion; this would be almost proved, if the quicksilver level, observed in distant places exactly at the same times, gave out the same variations in sign and in quantity: (B) otherwise these changes must be ascribed to the earth's surface; and this last supposition is the most probable, because earthquakes prove abundantly that the outer crust of our globe is not devoid of relative motion.

My largest fluctuation of the plumb-line amounted, in 1872, to $4''\cdot5$. Astronomers have observed greater deviations. In 1875, between March 9 and June 8, the Greenwich observations show, by γ Draconis, a change of $7''\cdot1$. Yvon Villarceau got a discrepancy of $6''\cdot9$ in his latitude of Strassburg, and quite recently Elkin had, in the same place, extreme results differing as much as $8''$. This quantity is of the same order as the famous difference of solstices which troubled astronomers so much at the beginning of the present century.

Henry and Ellis have recorded changes of level and azimuth in the fixed instruments of Cambridge and Greenwich, and I have often seen my nadir shifted in the prime vertical. These changes must affect absolute R.A. and longitudes by telegraph; for a transit instrument carefully levelled today, may not remain parallel to its previous position when levelled with equal care tomorrow. A change in the direction of the plumb-line, found here in the plane of the meridian, will, if affecting the whole earth, be observed as a change of azimuth in a place differing from this one by 6^h in longitude. Greenwich observations prove the existence of small changes in true azimuth. At all events corrections for these variations must be applied to geodetical measures, and we require at least a new definition for the vertical line, which is erroneously supposed unchangeable.

ANTOINE D'ABBADIE,
Membre de l'Institut.

1878, May 2.

Occultations of Mars.

As an occultation of the planet Mars by the Moon will happen on the 3rd of June*, a phenomenon that has not been visible in this country for twenty-one years, it may be of some interest to look up the impressions of observers on former occasions when the planet was occulted.

We might go back to the time of Tycho Brahe, who tells us he diligently watched an occultation of Mars on the night of December 30, 1595, when "luna Martis stellam parte opacâ operire cæpit."

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* 'Observatory,' No. 13—*Astronomical Memoranda.*

The planet was occulted on the night of February 18, 1837, the Moon passing the meridian during the phenomenon at an altitude of nearly 60°. Mr. Baily gives an account of his observation of this in the 'Monthly Notices' of the Royal Astronomical Society. He states that Mars was seen to touch the Moon at $8^{h}\ 45^{m}\ 2^{s}\cdot6$, and totally disappeared at $8^{h}\ 45^{m}\ 35^{s}\cdot6$. It was first seen to reappear at $9^{h}\ 54^{m}\ 0^{s}\cdot6$, and totally emerged $9^{h}\ 54^{m}\ 26^{s}\cdot6$. "The planet appeared of a fine yellow colour both at its ingress and egress. No projection was observed."

We gather from the 'Monthly Notices,' June 1846, that the occultation of Feb. 1 that year was observed at Oxford by W. Luff. Time by sextant; telescope 2·6 in.; immersion $9^{h}\ 37^{m}\ 37^{s}\cdot6$, emersion $10^{h}\ 17^{m}\ 38^{s}$.

A more interesting account is given of that on March 12, 1854, as observed by Mr. Snow, at Ashhurst, with 4 inches. "It was of almost precisely the same colour as the Moon," he says, "and in the most favourable moments I could not help comparing it to a spangle on the face of the sky. Whilst it was slowly and solemnly vanishing, it gave for several seconds the notion of its being the summit of a lunar mountain, but melting gradually away. At the emersion . . . . nothing whatever was to be seen of the two bodies clinging together, as it were, by threads of light; nothing of the pear-shaped appearance often recorded as put on by planets under similar circumstances."

An occultation of Mars, in daylight, was observed by Mr. Tebbutt, at Windsor, N. S. W., on August 12, 1875. He found the rapid disappearance of the planet's disk an exceedingly interesting phenomenon, its extinction taking place at a considerable distance from the Moon's illuminated disk. The planet was observed, at emersion, as a small projection on the Moon's limb just as she was getting out of a cloud. The limb of Mars being gibbous in form at the time, it was difficult to get the instant of last contact.

The last occultation of Mars visible in our own country was on October 14, 1857. S. J. Johnson.

Upton Helions Rectory, Crediton.

The Rev. Robert Main.

It is with much concern that we record the death of the Rev. Robert Main, M.A., F.R.S., for nearly 18 years the respected Director of the Radcliffe Observatory at Oxford, or, as he was there better known, the Radcliffe Observer. Mr. Main was born in the year 1808, and in due time he was placed under the tuition of a nonconformist clergyman at Portsea, to whose fostering care and skill as a geometer Mr. Main attributed his own strong taste for mathematical pursuits and his ultimate success as an astronomer. From

Portsea he removed to the Grammar School at Bishop's Waltham, in the capacity of assistant master; and chiefly from the savings of this not very lucrative post, and by a most commendable economy, he was enabled to place himself, in the year 1831, at Queen's College, Cambridge, where in 1834 he took the distinguished degree of sixth wrangler, and was soon elected to a Fellowship in the College. In the following year he went to the Royal Observatory Greenwich, as first assistant under the present Astronomer Royal. At Greenwich Mr. Main soon became distinguished for that orderliness, painstaking accuracy, general intelligence, and probity which formed a part of his character throughout his life. While at Greenwich, notwithstanding the continuous and responsible duties of his post, Mr. Main found time for the completion of many astronomical researches communicated to the Royal Astronomical Society, and for which he was ultimately awarded, as the highest mark of their approbation, the gold medal in 1858. In 1860 Mr. Main succeeded the late Mr. Johnson as Radcliffe Observer, in which capacity he continued to exhibit the same high qualities, moral and intellectual, for which he was respected at Greenwich. The regularity with which the annual volume of the Radcliffe Observations appeared is well known to astronomers. In 1863 he published an excellent book on astronomy, intended chiefly for students in our Universities. Perhaps it is not invidious to say it is the only book of the sort now extant which has been written by a real astronomer, and from which the student can learn real astronomy.

In the later periods of his life Mr. Main gave considerable attention to some of the religious controversies of the day, and he was favourably known at the Victoria Institute in London. It will be remembered that Mr. Main for many years served on the Council of the Royal Astronomical Society, of which body he became successively secretary and president. He married a sister of Professor Kelland, of Edinburgh, and leaves behind him a widow and three sons, who, together with a large number of colleagues and friends at Oxford and elsewhere, will long cherish his memory and deplore his removal from among them.

C. Pritchard.

Rand Capron's Photographed Spectra.*

This is the first work in which any extensive results of the application of photography to spectrum analysis have been put before the public. Although the instrumental means at Mr. Rand Capron's command were of the simplest, the photographs here presented tell us at a glance far more than is to be learnt from the most elaborate plates based on careful measurements with powerful spectroscopes. In fact it is now beginning to be recognized, thanks to the researches of Lockyer and others, that mere measurements of the positions of lines are not sufficient, and that each spectrum has an individuality of its own depending on the character of its lines. It is here that photography steps in. The breadth of the spectral lines depends on the density of the incandescent vapour which gives rise to them; and when a metal is vaporized by means of the electric spark, we can learn much as to the physical condition of the vapour from a knowledge of the breadth of the corresponding lines at different distances from the electrode. If there be impurities mixed with the metal we are examining, the vapours which they give off will not be so dense, and will not extend so far; and thus we shall be able to distinguish the true lines of the metal examined by their greater breadth in the neighbourhood of the electrode, and still more distinctly by their greater length, *i. e.* the greater distance to which they extend across the spectrum. Lines in the spectrum due to impurities will, on the other hand, be short, as the comparatively small amount of vapour will not extend far from the pole. It is by the application of this principle that Lockyer has sifted out, as it were, the true or long lines of each metal; and the necessity for some such process will be evident to any one who remarks the recurrence of the same lines in the spectra of several metals, notably in those of copper, lead, and zinc, as given in Dr. Watts's 'Index of Spectra.' Mr. Rand Capron's photographs show some very striking instances of the broadening of the lines in the immediate neighbourhood of the electrode. This is especially noticeable in the case of magnesium, though sufficiently marked in the spectra of many other metals, such as antimony, bismuth, indium, lead, nickel, thallium, zinc. On the other hand, barium and strontium, which rapidly melted away from the clips, show spectra with the lines broad and fuzzy in the middle, the vapour in this case having been dissipated at the electrodes by the heat of the spark.

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* Photographed Spectra—one hundred and thirty-six photographs of metallic, gaseous, and other spectra, printed by the permanent autotype process,—with Introduction, &c. By J. Rand Capron, F.R.A.S. London: E. & F. N. Spon, 1877.

But although, by applying the test of long or short lines, impurities may be sifted out, there still remain the air-lines, which commonly stretch right across the spectrum, and frequently constitute the most conspicuous part of it. This, however, is not an unmixed evil. The presence of these air-lines in most of Mr. Rand Capron's photographs constitutes, in fact, a natural scale to which the true metallic lines may be readily referred. As two photographs of the air-spectrum are given, it is easy to mark these off on a slip of paper, and by applying the slip to each of the photographs of metallic spectra to eliminate the air-lines.

Besides the spectra obtained with the induction-spark of a Ruhmkorff coil, in which these air-lines are generally found, Mr. Capron gives another set of photographs for which the electric arc between carbon points was used, and in all these photographs the lines and bands due to the carbon electrodes are more or less conspicuous. Now it is an important fact, brought out by the photographs, that the number and intensity of the air or carbon-point lines differ much in each spectrum, depending to a considerable degree on the metal burning in the spark or arc. "It is this fact," as Mr. Rand Capron remarks, "that seems to give to each spectrum its individuality." In the Introduction he specifies as important characteristics of the photographs:—

"1. That considerably more of the spectrum was obtained in the direction of the violet end than is ordinarily delineated in the published works on Spectra.

"2. That notwithstanding the moderate dispersion and the limited extent of the spectrum obtained (for want of the red end), each individual photograph had, without actual measurement of its principal lines, a separate and distinct character, easily recognized when once seen, and distinguishing it from all others."

It must be mentioned that Mr. Rand Capron used for the metals a direct-vision prism of an inch aperture, with collimator and camera, and with this spectroscope he obtained photographs of the spectra of some forty metals, extending from about *b* to H. For most of these metals two photographs are given—one taken with the induction-spark, and the other with the electric arc from a battery of 40 pint Grove cells. Amongst the spectra will be found two from meteoric iron and an iron meteorite respectively. In several cases the electrodes were of two different metals, the two spectra being thus presented for comparison.

For the spectra of gases, three different spectroscopes were employed, one of these having two simple quartz prisms of 60°, and the others each a direct-vision prism of large size.

A number of photographs of the spectra given respectively by the capillary and poles of Geissler tubes with various gases are presented, two spectra being in several cases brought together by means of the comparison prism. In the course of his work on the gases, Mr. Rand Capron made some experiments on the effect of an electro-magnet on the capillary part of the tube, but without any decided results. The electro-magnet was, however, hardly powerful enough, and photographic difficulties were experienced with the gases from which M. Chautard obtained his best results, viz. chlorine, bromine, iodine, sulphur, and tin chloride.

There can be no question, we think, that Mr. Rand Capron has done excellent service to spectroscopy by the publication of his very complete series of 136 photographs, together with the critical explanation which accompanies them. The book ought to be in the hands of all who use the spectroscope. In fact, the bringing together of so many spectra simultaneously before the eye gives a far greater insight into their physical characteristics than a long study with the spectroscope of the individual spectra one after the other.

Mr. Rand Capron is to be heartily congratulated on the success that has attended his efforts in a field which has too long lain fallow, and we trust that his work will be widely appreciated by all who take an interest in the revelations of the spectroscope.

EDITOR.

CORRESPONDENCE.

To the Editor of 'The Observatory.'

The Transit of Mercury, 1878, *May* 6.

SIR,—

The observation of this phenomenon at Oxford will, I fear, only add another instance to the list of failures in the south of England. A battery of seven telescopes had been prepared for the occasion, including the large Grubb refractor with its sun spectroscope, under the hope that we might catch the planet entangled on the C lines of the chromosphere. Dense clouds proved fatal to success, so far as observation of the ingress was concerned.

However, about nineteen minutes after the commencement of the transit, a few short and partial breaks in the clouds occurred, and during three brief intervals Mercury was steadily viewed in the De La Rue reflector of thirteen inches aperture. For the plane metallic speculum I had substituted a large Herschelian front surface reflector of glass; a Dawes's eyepiece having a diaphragm of 0·3 inch and a power of 120 was also applied, and the result was a calm and beautifully defined image of Mercury on the Sun. During two of the glimpses, I entertained no doubt that the centre of the planetary disk was very decidedly brighter than the rest of the planet; but during the third and shortest glimpse, I failed to recognize the same phenomenon. I could observe no aureola round the planetary disk. The conspicuous brightness near to or at the centre was confirmed by Mr. Jenkins, who assisted me with much intelligence during the observations. We both of us remarked that the disk of the planet was not so decidedly black as might have been expected.

Notwithstanding the unpropitious state of the sky, several Masters of Arts and some Heads of Colleges had assembled at the Observatory to witness the phenomenon. Some of them were rewarded by glimpses such as afore described; but in no case were the observations continued for a sufficient time, or with sufficient experiments, to enable me to speak with confidence as to the objective actuality of the bright stellar point at the centre of the planet's disk.

I am, Sir, yours faithfully,

C. PRITCHARD.

University Observatory, Oxford,
1878, May 18.

SIR,—

As it seems that the observations of the Transit of Mercury on the 6th were so few and so unfavourable, I send you the following notes of the ingress as seen by myself and three others here, with one reflector and three refractors (my own being of 6·3 in. aperture reduced to 2 in.):—The sun was clear of cloud from a few minutes before to a few minutes after ingress, the definition being good and unbroken. External contact was not before $3^h\ 12^m\ 0^s$, the average being $3^h\ 12^m\ 9^s$. Three of us agreed exactly as to internal contact being at $3^h\ 14^m\ 30^s$ G.M.T., and eight agreed within five seconds.

Yours truly,
JOHN J. COLE.

Mayland Sutton, Surrey,
14th May, 1878.

SIR,—

Referring to the transit of Mercury on May 6, we had the opportunity of observing it tolerably well here, being at our post at the time given in 'The Observatory,' and, watching intently, could find no trace of the planet outside the Sun's disk.

At $3^h\ 12^m\ 30^s$ G.M.T. saw the planet touch the sun's edge perfectly round, without any appearance of wing or arc of light. At the time of internal contact, the planet appeared to be elongated as if its edge was held by the Sun's; probably this was owing to a wisp of cirrus cloud which passed over the Sun at the instant. At 4.40 a very indistinct group of Sun-spots was to be seen in close proximity to the planet (like small grains of sand), which was not to be seen before or after the planet had passed it. One thing in particular struck me, and I followed it until 5.30, when the Sun was lost in threatening clouds—a clear white point of light, brighter than any portion of the Sun's disk, and not far from the centre of the planet.

I bade others who were with me look for it without telling them of its position, and they also picked it up. The telescope was a five feet achromatic by Cooke, aperture 4·1 inches; the powers used were 90 and 140.

Yours faithfully,
ROBERT MATHISON.

Innerleithen, Peebleshire,
1878, May 18.

The Companion to δ Cancri.

SIR,—

Sir John Herschel, in his 'Second Catalogue of Double Stars,' gives δ Cancri with a 15-magnitude distant companion as follows:—

$$P=160°\pm \quad D=25''\pm \text{ (estimated).}$$

Smyth includes this in the 'Bedford Catalogue,' calling the small star blue, which, he says, was "only seen by glimpses." He made the following measures:—

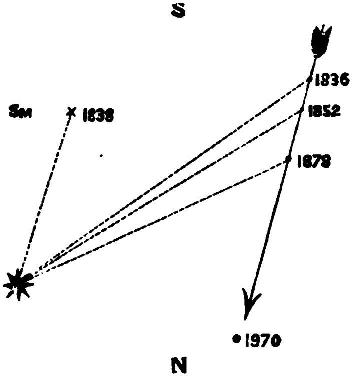
$$P=163°{\cdot}0 \quad D=25''{\cdot}0 \quad (1838{\cdot}3).$$

How the small star, "seen only by glimpses," but really not seen at all, if the measures have one half the value of a careful eye estimation, could have appeared blue or any other colour, requires some explanation.

The following are all the measures that have been made of the small star down to the present time. Fortunately one of them, the first ('Annals of the Munich Observatory,' Vol. xvii.), is an early observation, near the time of Herschel and Smyth, and settles beyond all controversy the position of the companion at that time.

Munich......	P=123°·8	Δ Decl.=27″·94	1836·1
Lassell	121°·1	D=45″·82	1852·3
Burnham ...	113°·9	D=40″·97	1878·2

The last measure was made with the 18½-inch refractor of the Dearborn Observatory. I have always found the small star very plain with my 6-inch. The accompanying diagram, drawn to scale,



shows the place of the companion at the different epochs. It shows that the motion is rectilinear, and that the change is certainly due to the proper motion of the large star, nearly in the direction of 164°, the apparent displacement of the small star, as shown by the arrow, being in the direction of about 344°. It also shows that the place assigned by Smyth is wholly erroneous. The nearest approach will occur not far from 1970, when the distance should be about 32″.

The proper motion of δ Cancri, according to the B. A. C., is R.A. $+0^{s}\cdot003$; N.P.D. $+0''\cdot24$. In the Radcliffe Catalogue the adopted proper motion in N.P.D. is the same, but in R.A. it is $-0^{s}\cdot002$. The observed positions of B give $+0''\cdot27$ in polar distance.

I am Sir, your obedient servant,

S. W. Burnham.

Chicago,
1878, April 28.

λ *Ursæ Minoris.*

Sir,—

I trust you will allow me a little space, just to add to what has been written touching λ Ursæ Minoris, that there is a long and very interesting note by Mr. Pogson on that star and the neighbouring variable Cephei 24 (Hevelii), in the 'Radcliffe Observations,' Vol. xv. page 291. I am indebted to the kindness of a well-known variable-star observer for this valuable reference. I am much obliged to your correspondents who have looked for λ with the naked eye; I wish I could conscientiously say I had been equally successful.

Yours truly,

GEORGE HUNT.

Hopefield, West Dulwich.
1878, May 9.

NOTES.

ON THE REVERSAL OF THE LINES OF METALLIC VAPOURS. No. II.*—In a second paper on this subject read before the Royal Society, Professors Liveing and Dewar give an account of further observations made by them. They have succeeded in reversing the characteristic lines of the vapours of rubidium and cæsium. On account of the volatility of these elements, of the small quantities of their compounds at the disposal of the experimenters, and of the light emitted from hot iron not extending so far into the violet as the rubidium lines, the method of experiment was varied from that described in their last paper.

In making the observations on the compounds of rubidium and cæsium, glass tubes were used arranged as follows:—A piece of combustion-tubing had one end drawn out, the end turned up sharply and sealed off so as to give a tolerably plane face at the end of the tube; "a small bulb was then blown at about an inch from the end, and the tube drawn out at about an inch from the bulb on the other side, so as to form a long narrower tube. Some dry rubidium or cæsium chloride was next introduced into the bulb, and a fragment of fresh-cut sodium, and the narrower part of the tube turned up, so as to allow the tube and bulb to be seen through in the direction of the axis of the tube. The open end was then attached to a Sprengel pump and the air exhausted; the sodium was then melted, and afterwards either dry hydrogen or nitrogen admitted and the end of the tube sealed off at nearly atmospheric pressure." This pressure of gas inside the tube was necessary to prevent the metal condensing in drops at the ends of the tube. A lime-light was viewed through these tubes placed lengthways in front of a spectroscope.

On warming the bulb of a tube in which rubidium chloride had been sealed up with sodium, the D lines were soon seen, and there appeared soon two dark lines near the extremity of the violet which were found to be identical with the violet lines of rubidium. Next appeared faintly the channelled spectrum of sodium in the green, and then a dark line in the blue (very sharp), in the place of the more refrangible of the lines of cæsium in the flame spectrum. As the temperature rose, these dark lines, those in the violet especially, became broader, and then a fine dark line appeared in the blue corresponding to the less refrangible of the cæsium blue lines. At this time no dark line could be seen in the red; but as the temperature rose, a broad absorption band appeared in the red, with its centre midway between B and C, ill-defined at the edges, and not very dark. The lines in the violet had now become so broad as to touch and form one band. On cooling, the band in the red became lighter gradually without becoming defined, and was finally overpowered by the sodium-spectrum in that region. The double dark line in the violet became again sharply defined as the temperature fell.

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* Proceedings Royal Society.

The two dark lines observed in the blue in the place of the characteristic blue lines of cæsium, Professors Liveing and Dewar believe to be due to a small quantity of cæsium chloride present in the rubidium chloride.

When a tube containing cæsium chloride and sodium was observed, the two dark lines in the blue were seen soon after the heating began, and the more refrangible broadened sensibly as the temperature rose. The usual spectrum of sodium appeared in the green, and an additional channelling appeared in the yellow, which may be due to cæsium or to the mixture of the two metals.

These absorption-lines of cæsium coincide with the blue lines of cæsium as seen in the flame, not with the green line which the metal shows when heated in an electric spark of high density.

It is to be noted, however, that by varying the electric means employed, the spectra of rubidium and cæsium chlorides may be made to vary.

The observations on the absorption of magnesium and of mixtures of magnesium with potassium and sodium have been extended. In these observations the same apparatus as described in the last paper was used.

The results are as follows:—When commercial magnesium (with only a small percentage of sodium) was used, the absorption produced by magnesium consists of—

(1) Two sharp lines in the green, one broader than the other, broadening further as the temperature increases, and coinciding in position with the least refrangible of the *b* group. The other is less refrangible and has a wave-length nearly 5215.

(2) A dark line in the blue. This line was better seen when a small quantity of potassium was added.

(3) A line or band in the green rather more refrangible than the *b* group. These absorptions may be fairly ascribed to magnesium. The following absorptions appear to be due to mixed vapours.

(4) When sodium and magnesium are used together a dark line appears in the green with wave-length about 5300. This is the characteristic absorption of the mixed vapours of sodium and magnesium.

(5) The characteristic absorption of potassium and magnesium consists of two dark lines in the red, with wave-lengths about 6580 and 6475.

(6) With a mixture of potassium and magnesium there was seen upon one occasion a dark line in the blue, with a wave-length about 4820. But this line may be due to magnesium alone.

The observations may be thus summed:—There is no exact reversal except of the line *b*, and this may be an accident. But in the solar spectrum there is an absorption line hitherto unaccounted for, closely corresponding to each of the lines above described. On Ångström and Thalen's map there are dark lines at 6580 and 6585, a broad dark line between 6474 and 6475, and a dark line at 5300. There are also dark lines nearly, if not exactly, coincident with the series of seven bright lines of magnesium detailed above, not seen strictly reversed.

The coincidences hitherto observed in the solar spectrum have chiefly been with lines given by dense electric sparks; it is not improbable that the conditions of temperature, and admixtures of vapours in the upper part of the solar atmosphere, may resemble much more nearly those in the tubes just described.

Professors Liveing and Dewar draw attention to the wide field for research in the absorptions produced by mixtures of vapours, and state their intention of pursuing their observations, using higher temperatures if possible. M. L. H.

THE NOVEMBER METEORS.—Prof. Kirkwood writes, in the American Journal of Science for 1878, January, that fifty-four Meteors were counted by two observers (B. Vail and J. P. Carr) at Bloomington, Indiana, on the morning of Nov. 14, 1877, between 1.55 and 3.45 A.M., being at the rate of 30 per hour. Nearly all were Leonids, a few being as large as first-magnitude stars, with trains which lasted for several seconds. The appearance of so large a number ten or eleven years after the maximum displays of 1866 and 1867 is quite unexpected. The early part of the night was cloudy, but before 2 A.M. the sky had become quite clear.

NEW DIRECT-VISION SPECTROSCOPE*.—M. Thollon has arranged a form of spectroscope in which "direct-vision" is obtained by a series of reflexions from right-angled prisms. The collimator and telescope are mounted in one straight line, and two right-angled prisms, united by their hypothenuses so as to form a perfect cube, are fixed between them, with their edges parallel to the slit, and one side of each facing the collimator and telescope respectively. The rays from the collimator are reflected laterally by the first of these two prisms, and fall on the upper part of a train of prisms placed on one side, and ending with a right-angled prism of two reflexions. By this arrangement the rays pass through the lower storey (as it were) of the prisms and cross the axis at right angles under the cube, when they fall on a similar train with a second reflecting prism, which sends the pencil back again through the upper storey and thence on to the second prism of the cube, by which it is reflected into the viewing-telescope. Half-prisms may be cemented on to the faces of the two reflecting prisms of the cube as in other spectroscopes, of which this arrangement is merely a modification. Another arrangement for giving direct-vision with an ordinary automatic train of prisms was contrived some time ago by Mr. Hilger of London. The rays, after passing twice through the train in the ordinary way, fall on a prism of two reflexions, which raises them above the plane of the prisms and sends them out in the original direction. In this form, the telescope and collimator are not in one straight line, though their axes are parallel; but, on the other hand, only four reflexions from two right-angled prisms are necessary instead of six from four prisms as in M. Thollon's plan.

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* Comptes Rendus, Vol. lxxxvi. No. 5.

POLARIZATION OF LIGHT FROM THE MOON AND VENUS *.—During the years 1872–5, Lord Rosse has been engaged with researches as to the position of elongation from the Sun at which the polarization of a point on the Moon's surface attains a maximum, under the idea that it might thus be possible to obtain an approximate value of the refractive index of the material composing the surface, and so to distinguish between material of a vitreous nature, ejected from volcanoes, and a surface of ice and snow. The subject has been invested with the greater interest from the fact that Arago, having found the maximum of polarization of the whole of the Moon's light to occur at or near quadrature, remarks on the circumstance as being what might be expected to result from the reflecting surface being gaseous; and he appears to think the polarimeter may afford us some information on the question of the existence of a lunar atmosphere.

A polarimeter differing little from Arago's was used, and the probable error of the value for the polarization on any one night, from a series of sixteen readings, did not exceed one per cent. But the discrepancies between various nights are much larger, and the results must be accepted with reserve. From observations on the Mare Crisium, ranging from an elongation of 60° to one of 150°, the polarization appears to be at a maximum for an elongation of about 86°, being then $\frac{1}{0{\cdot}780}$, whilst at 60° it is $\frac{1}{0{\cdot}830}$, at 120° $\frac{1}{0{\cdot}890}$, and at 150° $\frac{1}{0{\cdot}980}$. The amount of polarization is taken as the ratio of the intensities of the light polarized in and perpendicular to the plane through the Sun. Observations were also made on Mare Imbrium, Mare Serenitatis, Palus Somnii, the region between Macrobius and Proclus, and other parts. The polarization is in general greater on the plains than on the more uneven parts.

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* Proc. Royal Dublin Society.

Measurements of the light of Venus made between 1872, March 12, and April 6, gave as a mean value for the polarization $\frac{1}{0\cdot925}$, of which no regular variation was perceived during the progress of the observations, although the change of phase was considerable.

OXFORD UNIVERSITY OBSERVATIONS.—The University Observatory at Oxford, which owes its origin to Mr. De La Rue's generous gift of his 13-inch reflector and accompanying apparatus, was so far in readiness by the autumn of 1875 that observations were then commenced, and the first instalment of these is now published by Prof. Pritchard, to whose energy the establishment of this important Observatory and its complete organization are entirely due. Besides the De La Rue reflector which has been devoted to Lunar Photography, the Observatory possesses a fine equatoreal refractor of 12¼ inches aperture by Grubb, and the observations with this instrument up to the end of 1877 are here presented. They consist of three extensive series of micrometrical measures of Saturn's Satellites in 1875–6–7, of some 400 measures of about 120 double stars, and of observations of five comets in the year 1877. Of these, Comet *a* was observed on 17 nights, Comet *b* on 37 nights, Comet *c* on 16 nights, Comet *e* on 12 nights, and Comet *f* on 3 nights. Provisional elements of these five comets are given and, in the case of Comet *b*, a comparison of the observations with an ephemeris computed from the elements is made. Judging from the regularity of the exhibited discordances, it would seem that the accuracy of the observations approaches closely to that of the best star-measures. In conclusion Prof. Pritchard gives the elements of the orbits of three double stars (ξ Ursæ Majoris, 70 Ophiuchi, and μ^2 Boötis) computed by a new method, in which the results of a graphical solution are used as the foundation for an algebraic process. A comparison with the interpolation curve of the observations shows very small outstanding discordances, which, however, are to a certain extent systematic. It is hard to say how far these errors are due to personality, which would affect the results of several years at a time; but, at any rate, Prof. Pritchard has found that the results obtained by different observers are most conflicting, and on this account he has decided in future to restrict the observations to a small and select number of stars to be frequently observed and with great care. By means of the ingenious duplex micrometer recently devised by him, Prof. Pritchard hopes to be able to measure differences of celestial arcs up to 20′ with a degree of accuracy approaching that of the heliometer.

With the De La Rue reflector, 1200 measurable photographs of the Moon have been obtained, and these are now about to be measured with a delicate micrometer presented by Mr. De La Rue. By means of this instrument Prof. Pritchard hopes to be able to settle the vexed question of the Moon's physical libration.

The University of Oxford is to be congratulated on the success which has attended the operations of its new Observatory, in which a class of astronomical work that has long called for attention in this country is being taken up in a manner which gives every hope for the future.

SOLAR ERUPTIONS *.—P. Ferrari at Rome observed a remarkable solar eruption on 1877, Nov. 7, which appeared to be connected with the great spot then passing off the disk. The changes were so rapid at times that it was impossible to get accurate drawings of the form of the prominence, though P. Ferrari gives ten sketches made between 9.30 A.M. and 4.15 P.M. The most singular circumstance in connexion with this outburst was that the higher and lighter clouds of the prominence drifted from north to south as if carried by the wind, and so rapidly that they moved from 260° to 245° in a little less than half an hour. This fact, in P. Ferrari's opinion, confirms Secchi's observations of currents in the Sun's atmosphere analogous to our trade-winds. This prominence attained a height of 120″, and showed the lines of hydrogen, helium, iron, and 1474 K reversed. P. Ferrari remarks that on Nov. 3, whilst the spot was crossing the Sun's disk, there was a magnetic disturbance, and that such disturbances happen either before or after the appearance of a group at the east limb, and not merely at the exact time. We must, however, point out that the photographic records at Greenwich show this disturbance to have been of a very ordinary character. In fact it does not come up to the standard for a disturbed day; and there would seem to be nothing at all remarkable in the fact of a slight magnetic disturbance, such as may be expected on the average at least once a month, happening at some time during the fortnight when a spot is on the Sun's disk. A somewhat greater disturbance occurred on Oct. 11, when there was no trace of either spots or faculæ on the Sun; the magnets remained then very quiet till Nov. 3, notwithstanding the appearance of the spot on Oct. 24, after which there came another quiet period interrupted only by a small disturbance lasting from Nov. 8, 5^h, to Nov. 9, 10^h, *i. e.* from one to two days after the spot had passed off. The reappearance of the spot towards the end of November was not marked by any magnetic storm; the next which occurred being on 1878, Jan. 23–24, when, strange to say, the Sun was remarkably quiet, there being only two very small spots on the disk. Although there can be little doubt that solar phenomena and magnetic perturbations are in a general way related, it is very difficult to establish a direct connexion between a definite phenomenon on the Sun and a specified disturbance of the magnet. Our ignorance of what is happening on the other side of the Sun presents a great difficulty.

Mr. Trouvelot † records a remarkable instance of the sudden extinction of the light of a solar prominence which he witnessed on

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* Mem. degli Spettrosc. Italiani, 1877, November.

† American Journal, 1878, February.

1874, June 26. Whilst watching a very bright detached jet, resembling an elongated comma in shape and having a height of 3′ 37″, with a maximum breadth of 3°, it suddenly vanished and no further trace of it could be seen, although the small jets of the chromosphere below it remained visible. On several occasions Mr. Trouvelot has seen the light of a prominence die out gradually, and he has also witnessed the gradual illumination of a prominence—a similar phenomenon having been recorded by Prof. W. A. Rogers during the solar eclipse of 1869, observed by him with a 9-in. refractor at Des Moines, Iowa.

Another noteworthy observation is a sudden explosion seen on 1872, Sept. 10. At $0^h 33^m$ a narrow flame forming an arch 36″ high was seen; two minutes later it suddenly became detached from the limb and sprang up to a height of 70″, corresponding to a motion of 16,000 miles, or twice the earth's diameter. In another two minutes, at $0^h 37^m$, it had ascended to 118″, corresponding to a further movement of about 22,000 miles; at $0^h 43^m$, six minutes later, it began to descend, and in two minutes had reached its former level, soon after which it sank to the level of the chromosphere and was lost in it.

TRANSIT OF MERCURY.—In addition to the observations given in the Report of the R. A. S. meeting and in the *Correspondence*, reports have been received from a number of observers abroad. M. Janssen at Meudon was able to see Mercury outside the Sun's disk before external contact, and by means of the spectroscope he succeeded in establishing the existence of an atmosphere round the planet and its constitution. He also obtained some photographs, of which two are excellent. Capt. Mouchez and MM. Henry at the Paris Observatory observed internal contact, though the observation was bad owing to cloud, as was also the case at Algiers and Bordeaux.

At Toulouse M. Perrotin observed internal contact through cloud so dense that a dark glass was not used. Under these circumstances he remarked a *dark* aureole or penumbra round the planet, but this disappeared when the light became too bright.

The French expedition to Ogden, Utah, was perfectly successful, 78 photographs of the transit having been taken by MM. André and Angot, as well as observations of contact. Satisfactory observations and photographs were also taken at the U.S. Observatories at Washington and West Point.

The transit was also observed in Spain, at San Fernando and Cadiz. At the latter place M. Arcimis records that no arc of light was seen before internal contact, either outside the Sun or round the part of the planet's limb on the Sun. At internal contact the planet assumed a balloon or pear shape, and for a long time after no ring was seen round the planet. About an hour and a half after ingress, however, a dark ring, not so black as the planet and about 8″ or 10″ broad, was seen, the sky being then quite clear. No bright spot was detected. M. Arcimis used a 3¾ in. refractor, with powers of 92 and 230. He makes the interval between external and internal contacts $2^m\ 37^s$, whilst two observers at San Fernando make it $2^m\ 48^s$.

The observations of contact published up to the present time agree closely with the predicted times based on Le Verrier's tables, the general accuracy of which is thus supported.

SUPPLEMENT TO HERSCHEL'S GENERAL CATALOGUE OF NEBULÆ.—Since the publication, some thirteen years ago, of Herschel's great work, containing the places and descriptions of over 5000 nebulæ, a large number of new nebulæ have been found which should be added to the catalogue, whilst the observations of D'Arrest and others have revealed the existence of many discrepancies in the recorded observations. Thus Mr. Dreyer came to the conclusion "that this excellent work, both as regards completeness and exactness, appears to want a supplement." Remembering a remark of Sir John Herschel's that any amount of time spent in preparing extensive working-lists is well spent, Mr. Dreyer commenced his work at Lord Rosse's Observatory by bringing together a list of all objects which appeared to require re-observation, and in the course of the careful study of the work of other observers, especially D'Arrest and Schultz, to which he was thus led, a series of notes and corrections to the 'General Catalogue' was formed. This list of corrections, together with a catalogue of all the nebulæ (more than 1100) found since Herschel's work appeared, is now published. That such a catalogue of new nebulæ is much needed at the present time may be inferred from the circumstance that many of the "new nebulæ" discovered at Marseilles during the last seven or eight years are to be found either in D'Arrest's work or in Marth's list of 600 found at Malta with Mr. Lassell's four-feet reflector.

The Notes and Corrections to Sir John Herschel's 'General Catalogue' occupy 14 quarto pages, followed by a list of all published drawings of nebulæ not included in Herschel's list, after which comes the 'Supplementary Catalogue' containing the places for 1880 and summary descriptions of 1172 new nebulæ, thus raising the total number now catalogued to 6251.

ENCKE'S COMET.—Dr. von Asten of Pulkowa has published an ephemeris of this interesting object, founded on his elaborate discussion of the orbit ('Observatory,' Vol. i. p. 21); but unfortunately the distance of the comet from the Earth and the disturbing effect of twilight will hardly allow of observations before perihelion passage (July 11) in the northern hemisphere. The comet may, however, be satisfactorily observed after its perihelion passage at the southern observatories. EDITOR.

Astronomical Memoranda for 1878, *June* 15–*July* 14.

June.	G. M. T.	PHENOMENA.
	h m	
15. S.	15 26	φ Sagitt. Oc. R. 236°
16. **S.**	11 28	h^2 Sagitt. Oc. R. 197
	11 31	h^1 Sagitt. Oc. R. 252
	12 8	♃ 1 Sat. Sh. I.
	12 25·6	♃ 2 Sat. Ec. D.
	13 1	♃ 1 Sat. Tr. I.
	13 17·3	♃ 3 Sat. Ec. D.
	14 28	♃ 1 Sat. Sh. E.
17. M.	12 35	♃ 1 Sat. Oc. R.
	18	♃ ☌ ☾ ♃ 0° 11′ N.
18. Tu.	10 25	♃ rises.
	11 40	♃ 2 Sat. Tr. E.
	12 24	♄ rises.
	13 50	♀ rises.
	14 31	♃ 4 Sat. Tr. E.
	14 46·3	♃ tr. D. 19° 15′ S.
	15 44	☉ rises.
	18 10·6	♈ transits.
	18 22·8	♄ tr. D. 1° 6′ S.
	21 11·5	♀ tr. D. 14 45 N.
	22 49·5	☿ tr. D. 20 58 N.
19. W.	0 1·0	☉ tr. D. 23 27 N.
	8 18	☉ sets. No real night.
	15 13	θ′ Aquar. Oc. R. 42°.
20. Th.	16 15	7986 Oc. R. 272°.
22. S.	4	☾ Apogee.
☾	7 15	
23. **S.**	14 2	♃ 1 Sat. Sh. I.
	18	♄ □ ☉
	21 14·5	♀ tr. D. 16° 20′ N.
	23 10·5	☿ tr. D. 22 55 N.
24. M.	11 19·3	♃ 1 Sat. Ec. D.
	14 21	♃ 1 Sat. Oc. R.
25. Tu.	11 32	♃ 1 Sat. Tr. E.
	12 38	♃ 2 Sat. Sh. E.
	14 1	♃ 2 Sat. Tr. E.
27. Th.	13 29	♃ 3 Sat. Tr. E.
	14 29	χ′ Taur. Oc. R. 274°
28. F.	9 44	♃ rises.
	11 45	♄ rises.
	13 38	♀ rises.
	14 3·4	♃ tr. D. 19° 29′ S.
	15 48	☉ rises.
	17 31·3	♈ transits.
	17 44·4	♄ tr. D. 1° 0′ S.
	21 18·0	♀ tr. D. 17 47 N.
	23 36·4	☿ tr. D. 24 7 N.
29. S.	0 3·1	☉ tr. D. 23° 15′ N.
	8 19	☉ sets. No real night.
	18	☿ Perihelion.
30. **S.**		
●	0 31	
July.		
1. M.	13 13·6	♃ 1 Sat. Ec. D.
	21	☉ Apogee.
2. Tu.	0	♂ ☌ ☽ ♂ 0° 33′ N.
	10 57	♃ 1 Sat. Tr. I.
	12 19	♃ 2 Sat. Sh. I.
	12 44	♃ 1 Sat. Sh. E.
	13 17	♃ 1 Sat. Tr. E.
	13 23	♃ 2 Sat. Tr. I.
3. W.	21 22·0	♀ tr. D. 19° 5′ N.
4. Th.	0 4·4	☿ tr. D. 24 14 N.
	1	☿ Sup. ☌ ☉
	10 44	♃ 2 Sat. Oc. R.
	11 13	♃ 3 Sat. Sh. I.
	13	☾ Perigee.
	13 12	♃ 3 Sat. Tr. I.
6. S.	8 24	4201 Oc. D. 115°.
☽	20 20	
8. M.	9 2	♃ rises.
	11 7	♄ rises.
	13 19·7	♃ tr. D. 19° 47′ S.
	13 33	♀ rises.
	15 56	☉ rises.
	16 52·0	♈ transits.
	17 6·2	♄ tr. D. 0° 59′ S.
	21 26·5	♀ tr. D. 20 12 N.
9. Tu.	0 4·9	☉ tr. D. 22 22 N.
	0 31·2	☿ tr. D. 23 13 N.
	8 14	☉ sets. No real night.
	12 18	♃ 1 Sat. Sh. I.
	12 41	♃ 1 Sat. Tr. I.
	14 38	♃ 1 Sat. Sh. E.
10. W.	12 16	♃ 1 Sat. Oc. R.
11. Th.	12 58	♃ 2 Sat. Oc. R.
13. S.	8 43	6562 Oc. D. 93°.
	11 47	♃ 4 Sat. Oc. R.
	21 31·4	♀ tr. D. 21° 7′ N.
○	22 55	
14. **S.**	0 54·4	☿ tr. D. 21 15 N.
	15 4	4 Capr. Oc. D. 113°.
	21	♃ ☌ ☾ ♃ 0° 18′ N.

EDITOR.

Meteor Notes for June.

Our tables of radiant-points (chiefly compiled from Mr. Greg's valuable catalogues), which have now extended over 12 months, contain, it is believed, a large number of the most important annually-recurring meteor-streams, and will, it is hoped, afford such information for each month as is required by amateurs. It is not proposed to continue these tables in the same form, as that would merely result in useless repetition; but our future references to this branch will contain notices of those radiants not already mentioned, and will for the most part consist of notes on new showers and directions to intending observers. We also hope to give extracts from recent observations, and shall be glad to receive such records from any of our readers engaged in the work.

In June shooting-stars are more than ordinarily frequent, and notwithstanding the strong twilight should be looked for, in the absence of the moon, during the first 12 days chiefly in the mornings, and during the last 8 days chiefly in the evenings. It is desirable to reobserve and confirm showers at 286°, +21°; and 273°, −2°. The latter (close to η Serpentis) falls near the radiant and date of Comet III. 1618. There is also a shower near η Ursæ Majoris at 207°, +47° (No. 72 of S. & Z.) for June 14–21, which has, however, already been verified by Heis. A strong radiant at 285°, −13° was observed by Tupman for June 24–July 6, and slightly confirmed by the writer last year from several large slowly-moving meteors. Showers at 242°, +12° (Heis, June); 255°, +23° (Schmidt, June 15–26); 260°, +54° (Heis, June 16–30); 266°, −12° (Schmidt, June); and 330°, +42° (Heis, June), should be watched for. The latter is the *Lacertids*; G. & H. give the radiant slightly N.E. of Heis. It is a very rich shower with long duration, and apparently endures through July into August. It will be important to ascertain the exact positions and durations of these successive showers from Lacerta. In this connexion it is obviously essential to utilize only such meteors as are well seen in regard to the direction and length of path and other specialities of appearance.

Recent Observations. The April Lyrids.—A few of these meteors were seen on April 20, 21, and 22, at Bristol. Watches reaching 5½ hours in the aggregate on the 3 nights revealed 54 shooting-stars, of which 13 were *Lyrids*. The radiant-point was very sharply defined at 272°, +32°, and evidently lies very close indeed to that of Comet I. 1861, with which a connexion has been inferred. The meteors were trained, with slow motion and foreshortened paths. In 1873–74 the absolute inaction of this radiant was noticed on April 21, while this year it certainly continued in progress on that and succeeding night. Of the contemporary showers only two could be ascertained with any certainty, viz. at 293°, +43°; and 263°, +62°. Only 2 meteors were conformable to a radiant in Corona.

W. F. Denning.

Position of the Sun's North Pole.

1878.	Pos.-angle. ° ′	Distance. ° ′
June 19	352 7	88 15
24	354 20	87 40
29	356 37	87 7
July 4	358 55	86 34
9	1 11	86 3
14	3 26	85 34

The Moon.

1878.	Rises. h	m	Geoc. Libr. at midnight. long. °	lat. °	1878.	Sets. h	m	Geoc. Libr. at midnight. long. °	lat. °
June 16...	10	6	+5·26	+2·74	July 1...	9	20	−3·73	−1·73
17...	10	33	4·83	+1·32	2...	9	45	2·84	−0·09
18...	10	51	4·09	−0·13	3...	10	4	1·85	+1·58
19...	11	4	3·07	1·54	4...	10	18	−0·79	3·15
20...	11	18	1·87	2·87	5...	10	32	+0·29	4·53
21...	11	29	+0·55	4·06	6...	10	47	1·35	5·62
22...	11	40	−0·78	5·07	7...	11	2	2·37	6·37
23...	11	52	2·05	5·86	8...	11	22	3·28	6·73
24...	12	6	3·18	6·41	9...	11	48	4·07	6·69
25...	12	22	4·10	6·68	10...	12	22	4·68	6·26
26...	12	46	4·76	6·64	11...	13	12	5·07	5·49
27...	13	19	5·12	6·27	12...	14	14	5·22	4·42
28...	14	5	5·08	5·57	13...	Full.		5·11	3·15
29...	15	8	4·94	4·55	14...	Rises.		4·72	1·73
30...	New.		−4·44	−3·25	15...	8	55	+4·07	+0·26

A. M.

The Planets.

Mercury is a morning star till July 4, when he is in superior conjunction with the Sun.

Venus is a morning star, rising before 14^h. Her disk is gibbous, the illuminated portion on June 15 being 0·689, and on July 15 0·790, and the corresponding diameters 15″·9 and 13″·2.

Jupiter is a morning star, in Capricornus, approaching opposition. His diameter on June 15 is 42″·7, and on July 15 45″·0.

Saturn is a morning star, stationary, about midway between β Ceti and γ Pegasi. The following are the dimensions of the ring:—

	Outer Ring. Maj. Axis.	Outer Ring. Min. Axis.	Inner Ring. Maj. Axis.	Inner Ring. Min. Axis.	Ball. Diam.
June 14	38″·78	3″·19	25″·79	2″·12	15″·6
July 4......	40″·16	3″·46	26″·70	2″·30	16″·2

The south side is visible, the elevation of the Earth above the plane of the rings being 4° 43′ and 4° 57′ S. on June 14 and July 4, and of the Sun 1° 54′ and 2° 12′ S. EDITOR.

Ephemeris of Tempel's Comet (by Dr. L. SCHULHOF).

Paris Midnight.	R. A. h m s	Dec. ° ′	Log. Δ.	Brightness.
June 17	15 33 25	4 43 N.		
19	15 32 15	4 17	9·8192	0·947
21	15 31 13	3 49		
23	15 30 20	3 20	9·8157	0·988
25	15 29 38	2 49		
27	15 29 5	2 15	9·8133	1·025
29	15 28 42	1 39		
July 1	15 28 31	1 1 N.	9·8119	1·058

The brightness was 0·385 at the time of the last observation of the comet on 1873, Oct. 20.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 15. JULY 1. 1878.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 14th June, 1878.

Prof. Cayley, M.A., LL.D., F.R.S., *Vice-President*, in the Chair.

Secretaries: Mr. J. W. L. Glaisher, F.R.S., and
Mr. A. Cowper Ranyard.

The Minutes of the last Meeting were read and confirmed.

Mr. Glaisher announced that 50 presents had been received by the Society since the last Meeting, and thanks were voted to the donors.

Professor Adams read a paper entitled "Note on a remarkable property of the analytical expression for the constant term in the reciprocal of the Moon's radius vector."

The following is a brief outline of the results obtained in the paper:—It is known, *à priori*, that if the mean distance in an undisturbed elliptic orbit which the Moon would describe about the Earth in its actual periodic time be taken as the unit of length, and if the reciprocal of the Moon's radius vector be developed in circular functions of angles which increase proportionally to the *time*, then the constant term in this reciprocal will be of the form

$$A + Be^2 + C\gamma^2 + Ee^4 + 2Fe^2\gamma^2 + G\gamma^4 + \&c.$$

Where e is the eccentricity of the Moon's orbit, and γ the sine of half the mean inclination; and the coefficients A, B, C, &c. are functions of m, the ratio of the mean motions of the Sun and Moon, and of e'^2, the square of the eccentricity of the Sun's orbit.

Plana, and those who have followed him in the analytical treatment of the Lunar theory, have developed the functions of m which occur in the coefficients of the lunar inequalities, in series of ascending powers of m, and have determined a few of the leading terms in these series.

With respect to the coefficients B and C in the above expression, Plana shows that if the eccentricity of the Sun's orbit be neglected, and if the developments be carried to terms involving m^3, then these coefficients both vanish. Pontécoulant afterwards showed that the same thing takes place when the developments are carried to terms involving m^6.

These investigations of Plana and Pontécoulant, while they show that the coefficients of certain powers of m vanish by the mutual destruction of their parts, give no reason why such mutual destruction takes place, and throw no light on the values of the coefficients of higher powers of m. The author was led, many years ago, to consider this subject from a new point of view; and he succeeded in proving, by two different methods, that the functions denoted by B and C vanish identically, whatever powers of m and e'^2 are taken into account. The simpler and more elegant of these methods is given in the present paper.

By an extension of the same method the author has found two simple relations between the coefficients E, F, G, of the terms involving e^4, $e^2\gamma^2$, and γ^4 respectively. These relations may be thus stated.

If, as usual, c denote the ratio of the Moon's mean motion from the perigee to its mean sidereal motion, and g denote the ratio of the Moon's mean motion from the node to the same mean sidereal motion, and if the terms of c which involve e^2 and γ^2 be

$$\mathrm{H}e^2+\mathrm{K}\gamma^2,$$

and similarly the terms of g which involve e^2 and γ^2 be

$$\mathrm{M}e^2+\mathrm{N}\gamma^2,$$

then the relations in question will be

$$\frac{\mathrm{E}}{\mathrm{F}}=\frac{\mathrm{H}}{\mathrm{K}} \text{ and } \frac{\mathrm{F}}{\mathrm{G}}=\frac{\mathrm{M}}{\mathrm{N}},$$

where H, K, M, N, like E, F, and G, are functions of m and e'^2.

Mr. Neison. I have been much interested by Prof. Adams's paper, for he has solved a problem of which I have only succeeded in obtaining a partial solution. I have succeeded in showing that, as far as the second power of the disturbing forces, the coefficients of these terms in the constant part of the moon's parallax must vanish. Therefore there can be no term lower than the order m^6. I have not yet been able to extend this proof to the higher powers of the disturbing forces. Prof. Adams, however, has now shown that these coefficients must always vanish.

Captain Tupman read a paper on the measurements of the Transit of Venus photographs. He said that the photographs which had been measured were taken with the five Photoheliographs made by Mr. Dallmeyer for the Transit of Venus Expeditions on "Patent Plates" 6 inches square, the images of the Sun being very nearly 3·9 inches in diameter. The dry process of Captain Abney was used throughout.

The measuring instrument, the determination of the errors of its glass millimeter scale, and the method of obtaining the optical distortion of the Photoheliographs had already been described in the Society's 'Proceedings.' It had been found by an elaborate investigation that the lines of equal distortion were sensibly circles con-

th

centric with the centre of the field. The actual correction for distortion for that zone of the field in which the points to be measured generally fell was exhibited on the board and was almost identical for all five instruments.

Before commencing the measures of a negative, the position of the line of centres was marked upon the film by a simple mechanical process. This operation had been performed independently by Mr. Burton and himself with no sensible difference. He had paid no attention to the marks left by Mr. Burton on the plates and found that his own coincided with them in direction.

In placing the negative in the instrument the circular carrier was turned about until the line of centres was truly parallel to the direction of the sliding motion of the microscopes.

When the negatives are placed under the microscope with an amplification of only 5 or 6 diameters, the limbs of both planet and Sun, even those which are pretty sharp to the unaided eye, become extremely indistinct, and the act of bisecting a limb with the wire or cross of the micrometer is mere guesswork. The deposit of silver fades off gradually to nothing, and the denser the film the broader generally is the zone of fading off, and the more uncertain the measures. In many cases the difficulty is aggravated by ruggedness due to atmospheric disturbances; but the smooth and gradual fading off is the chief cause of uncertainty.

There is only *one* really sharp picture in the whole collection, including the Indian and Australian contingents, and that is one of Captain Waterhouse's wet plates taken, at Roorkee, with a Dallmeyer instrument precisely similar to the others.

It should be remarked that in these instruments the artist has attempted to unite the photographic and visual foci on the collodion film. No doubt some sharpness of the photographic image was thus sacrificed; but this has little or nothing to do with the unfortunate failure of the photography generally.

Each photograph had been measured six times by Mr. Burton, and six times by himself. He was not able to include in his series of measures all the photographs measured by Mr. Burton, for the reason that when some of them were viewed through the microscope he could see nothing to bisect, either from the extreme faintness of the film or from its too gradual fading off. Mr. Burton generally employed a cross of webs, but he had preferred a single very fine web, the breadth of which was eliminated in the mean by the mode of bisecting.

It had been suggested that the measuring instrument should possess the power of rotating the sun's image about a mechanical centre. This would be useful in some cases of rugged limbs when the sun's image was not rendered elliptical by refraction, but, in his opinion, would make no material difference in the accuracy of measurement. The rotation could only be applied to the limbs of the sun, whereas, perhaps, the greatest difficulty had been at the limbs of the planet.

From the measures corrected for distortion were obtained the photographic diameters of the Sun and of *Venus*; the former presumably enlarged, the latter diminished by irradiation in a sensibly equal degree. The sum of the measured diameters in millimetres was compared with the sum of the tabular diameters, subject to errors for the scale value, and thus every photograph furnished its own scale. The paper then gave the formulæ by which the measured distance of centres affected by errors of semidiameters was compared with the tabular distance affected by errors of parallax, Right Ascension, and North Polar Distance. From each photograph was formed an equation involving all the unknown quantities, of which the errors of parallax and of semidiameters were the more important.

The rigorous solution of the equations resulting from Mr. Burton's measures was:—

$$\begin{aligned} \text{Mean Solar Parallax} &= 8''{\cdot}240 - {\cdot}021\,(d\mathrm{R} + dr). \\ d\,\mathrm{R.A.} &= +5''{\cdot}38 + {\cdot}287\,(d\mathrm{R} + dr). \\ d\,\mathrm{N.P.D.} &= -5''{\cdot}10 - {\cdot}882\,(d\mathrm{R} + dr). \end{aligned}$$

From his own measures he obtained

$$\begin{aligned} \text{Mean Solar Parallax} &= 8''{\cdot}082 - {\cdot}040\,(d\mathrm{R} + dr). \\ d\,\mathrm{R.A.} &= +5''{\cdot}04 + {\cdot}298\,(d\mathrm{R} + dr). \\ d\,\mathrm{N.P.D.} &= -3''{\cdot}36 - {\cdot}881\,(d\mathrm{R} + dr). \end{aligned}$$

The parallax deduced being absurdly small, altogether inadmissible indeed, the Astronomer Royal suggested that the quantity $(d\mathrm{R} + dr)$, or the sum of the corrections to the Tabular semidiameters, should be considered the only unknown, and that approximate values of the true Solar parallax and of the errors of R.A. and N.P.D. should be substituted in the equations. This was done, the mean Solar parallax being taken at $8''{\cdot}85$, $d\,$R.A. as $+5''{\cdot}81$, $d\,$N.P.D. as $-5''{\cdot}33$, which values resulted from the general solution of the whole of the contact equations, and the following values of $d\mathrm{R} + dr$ were obtained:—

Station.	By Burton's Measures.	Number of Photographs measured.	By Tupman's Measures.	Number of Photographs measured.
	″		″	
Luxor …………	−0·96	11	−0·13	12
	−1·18	11	−1·61	11
	−2·21	11		
Honolulu ………	−1·29	11	−0·21	12
	−1·19	11	−0·54	12
	−0·71	10		
Rodriguez………	+1·19	11	+2·44	9
	+0·33	10	+2·49	6
	+2·10	10	+2·74	8
	+0·14	11	+1·27	10
	+1·46	11	+2·31	9
Burnham, N. Z…	+0·68	13	+1·89	3
			+1·38	10
Kerguelen………	+1·51	8	…	0

The above is perhaps the best way to exhibit the nature of the discordances; they might also have been shown as apparent errors of the tabular distance of centres.

The discordances at any one station are too large to admit of the measures being employed with advantage for the determination of the Solar Parallax. They are due to inherent defects of the photographic images. The reason why at the two northern stations the signs are all *minus*, while at the three southern they are all *plus* is at present obscure, and he (Captain Tupman) was not prepared to offer any suggestion as to the cause.

The Astronomer Royal. I will confine myself to one or two supplementary remarks. We took very great pains in regard to all things which would seem of the nature of constant error in the measures of the photographs. The first thing was to assure ourselves of the accuracy of the millimeter-scale engraved by Mr. Simms, by means of which all the measures made upon the photographs are evaluated. We made the most careful examination by a process of halving and halving. We took a space of 128 divisions and dividing by 2 we had 64, and by comparing these two halves we got the error of the middle point. This space of 64 was divided into two, so that we got the error of each 32nd division. Then, again, that was divided into two and we got the error of each 16th division, and so on until the error of every division of the engraved scale was found with the utmost accuracy.

Another thing was to get an assured measure of the distortion in the direction of the radius of the field; and for that purpose we made use of a scale of equal parts 16 feet long, very carefully constructed by Mr. De La Rue for another purpose. It is a beautiful piece of mechanical structure, but we did not trust to the equality of the divisions there; we checked them every one by accurate measurements, relying only upon the firmness and good construction of the instrument. This scale of equal parts was made by Mr. De La Rue some years ago, and it was carefully preserved at the Kew Observatory, and it has turned up for a use even more important than that for which it was made originally. We took photographs of that scale in the same manner that we did for the Sun and Venus, and we were prepared then to correct the measure of the image of every portion of every one of these photographs of the scale; and by having a photograph of the scale which, by correction, became a scale of equal parts, and by examining by means of the millimetre scale we found those which ought to be equal parts in the photographs were not equal parts, and that was the effect of distortion. It was not very great; but it came out uniformly, measure after measure, in such a way as showed that our process was one of at least sufficient accuracy.

With reference to the result, I must say I was grievously disappointed. I had been officially called upon to express an opinion as to the propriety of incurring the expense of the photographic work. Although I had some doubts upon it, yet, upon the whole, I expressed myself as favourable to it, and was responsible for the expense. I can only say that I have been much disappointed. The images themselves certainly are very troublesome. I think the discordances will be quite understood from what was shown upon the board. The millimetre measure of the distance between the centre of the Sun and the centre of Venus in the photograph is a certain thing, inasmuch as it is founded upon a point which is the mean between the two limbs of the Sun, and the point which is the mean between the two limbs of Venus. I have no doubt of the generally close accuracy of that measure. Then the thing was to get a scale for the evaluation of that measure, and the scale which I adopted (I do not say it was infallible, but it was the best I could use) was the sum of the diameters of the Sun and of Venus. The reason for that was, that in taking the measure of the diameter of the Sun we are affected by the errors of irradiation of one kind, and in taking the measure of the diameter of Venus, which is black upon white, we were affected by errors of irradiation of an opposite kind. We might suppose that they would annihilate each other, but I am confident now that they did not annihilate each other. The uncertainty and the discordance of our results, I imagine, arises from two causes; one is from the excessive brightness about Venus which sometimes makes Venus look almost like a hat with a verge around it, and from the excessively gradual degradation of light at the limb of the Sun. It is a far more difficult practical problem than I could have imagined before the observations were made.

The first result is that we get a parallax very much differing from that which is obtained from the eye observations. From the eye observations, after the most careful discussion by Captain Tupman and myself, we are inclined to fix upon something like $8''\cdot83$. I will not answer for the one-hundredth part of a second. It is not the quantity which we fixed on at first, but that on which, after careful discussion, we are inclined to fix. From the reduction of those photographic measures we get a parallax of something like $8''\cdot2$, a thing which is utterly irreconcilable with that derived from the eye-observations. Such a discordance as $0''\cdot6$ in the amount of parallax would spoil every observation made by the eye, and would introduce the most glaring inconsistencies. Under this difficulty I suggested to Captain Tupman a course which he has adopted—to see if we could make out any thing like an approximate, I cannot say cause, but a connexion between those errors and the circumstances of observation in any way; and the way in which I proposed to do it, and which was carried out, was this:—I took for granted that the reduction of the eye-observations, as given, is very accurate, I will not say certain, and of indubitable accuracy, but affording very accurate values for the various elements which occur, namely, the solar parallax, the relative error of the place of Venus in right ascension, and the relative error of Venus in polar distance. These, combined with the value of the parallax which we have found, will give us a very constant set of values for the sum of the radii of

Venus and the Sun. If we take those same values, which we consider to be certainly established, and apply them to the equations which represent the photographic results, we get for the sum of the values of the radii of the Sun and of Venus those large discordances which were marked out on the board by Captain Tupman, and that is all that we can say. We believe that at certain stations the diameter of the Sun or of Venus, or both, came out too large, and that at certain other stations it came out too small. Why I cannot tell at all. I do not see any sufficient explanation of it, although I have no doubt whatever of the fact. That is the state in which we are left, and I am very sorry for it.

Prof. Cayley. In one of the photographs, I think you said, the border comes out quite sharply.

Capt. Tupman. That was a wet plate taken by Captain Waterhouse in India.

Mr. Gill. Although we can hardly say that observation by photography is any thing but an experiment as yet, it must be, in a certain sense, satisfactory to the Astronomer Royal to find that, so far as reports reach us, the other nations who have adopted photography in the Transit of Venus observations have not been more successful than have the British. For my own part I attached a good deal of importance to the absolute determination of the scale-value of our photographs, and great pains were taken to secure an absolute determination, independent of diameters; but it seems that the American and also the French photographers have not been more successful in this than their English colleagues; so that there is some subtle cause to be searched for, which is reserved for the photographic observers of the future, and which certainly deserves attention, if the Transit of Venus is to be observed by photography on a future occasion.

Mr. Brett. The Astronomer Royal has spoken about the expense of getting these photographs. It seems to me that the money has been very well expended, if it has at all events ascertained it as a fact that the limb of Venus upon the Sun cannot be seen, which is what I should have expected.

Mr. Common. In taking a photograph of the scale to which measurements were applied, was the focus of the photoheliograph altered in any way?

Capt. Tupman. The secondary magnifier was not disturbed. The object-glass was, of course, a little in front of the position it occupies when the celestial focus is used.

Mr. Common. Would not that introduce error?

Capt. Tupman. I think not, or a very small error indeed, because the distortion is nearly all due to the secondary magnifier.

Mr. Common. The focal image of the Sun falls a certain distance from the secondary magnifier. In taking an enlargement from an object which is not at an infinite distance away, the focus has to be so altered that the distance between the secondary magnifier and the focal image is materially altered.

Capt. Tupman. No, that is not altered, that remains constant. The correction is made by pushing the object-glass further away.

Mr. Common. There is one distance from the object-glass at which all celestial objects will focus, and to get an enlargement of the Sun by the secondary magnifier for all celestial objects that secondary magnifier will be a certain distance from this focal plane. In taking an enlargement of the scale by which you test the errors of the instrument, do you alter the distance of the secondary magnifier from this focal plane or not? because, if you do, you must make fresh errors.

Mr. Gill. You simply alter the divergence of the rays upon the secondary magnifier to some extent.

Capt. Tupman. I conceive there is no sensible error from that cause. There might be something very minute, but the focal image of the scale by which the distortion is determined occupied the same place in the tube that the solar image did, because the object-glass is moved away from the secondary magnifier by the necessary amount.

Mr. Ranyard. The lenses of the magnifier were not shifted?

Capt. Tupman. The secondary magnifier was used under precisely the same circumstance as during the transit with regard to its position between its foci.

Capt. Tupman read extracts from a paper on the Mean Solar Parallax derived from the telescopic observations of the Transit of Venus, 1874.

The first part of the paper consisted of *verbatim* copies of those portions of the observer's original reports relating to the Internal Contacts, which had not been hitherto published. They were given in order that astronomers might judge of the way the subject had been treated. He no longer believed in the possibility of maintaining the special phases of Internal Contact adopted in the Parliamentary Report, which were founded only on some of the British observations. He had confined himself in the present investigation to the phases *Contact Complete* for both ingress and egress.

Treating the Ingress first, he had selected the times which seemed to him to correspond as nearly as possible to the phase of Ingress Complete. The language of the observer was not always satisfactory as regards the definition of the phase. The parallax in reality depended upon the British observations, and, unfortunately, it was possible to take very different views of the observations in the Sandwich Islands. He accepted Mr. Stone's views of these observations.

The equations formed from the selected times were freed from the effects of errors of R.A. and N.P.D., which means simply that the observations are referred to the true geocentric position of Venus on the Sun's disk.

In the first solution every observation was given equal weight with the result for Mean Solar Parallax of 8″·863. He, however, thought it proper to give double weight to the observations made with the larger instruments under favourable atmospheric conditions, and to reject a few very wild ones, chiefly by amateurs, which were unimportant from the extreme smallness of their parallax factors. He then obtained a Mean Solar Parallax of 8″·845; but he considered that the observations were fairly satisfied by any value of the parallax between 8″·84 and 8″·87. The number of observations upon which this result practically depended was 14, 4 of which had received double weight. The residuals, being expressed in seconds of time as apparent errors of observation, were

in 9 cases under 2^s,
in 4 others under 5^s,
in 4 others under 8^s,
in 3 others under 12^s;

while the residuals of the rejected observations ranged from 17^s to 30^s.

The Egress was then treated in a similar manner. There were in all 48 observations. Of these, 2 were rejected because there was no guarantee for the local time, and 5 because they appeared to be at least 30^s in error. Of the remaining 41, he considered 14 entitled to double weight; and, thus weighted, the solution of the equations gave the Mean Solar Parallax 8″·846, showing an unexpected agreement within ·001 with the Ingress result. It was, however, to be remarked that there were 4 observations at Sydney, N.S.W., and one in India, which had very great weight in producing a small parallax. If these observations were not included, the solution of the remainder, weighted as before, gave 8″·89 for the Solar Parallax; but on this supposition the Sydney observations would be about 17^s too late. There was no reason whatever for rejecting the Sydney observations—on the contrary, he had given Mr. Russell's double weight. On the whole, he thought the Egress observations were fairly satisfied by any value of the parallax between 8″·83 and 8″·88, although something between 8″·84 and 8″·86 was most probable.

The residuals for the Egress observations, expressed as before, were

in 20 cases under 5^s.
in 14 others under 10^s,
in 6 others under 13^s,
in 1 other under 15^s.

The apparent errors of 6 of the rejected observations ranged from 16^s to 48^s, the remaining one was 90^s in error.

The general mean of Ingress and Egress, viz. 8″·8455, corresponds to a mean distance of the Sun of 92,400,000 miles.

Mr. Ranyard read a paper by Prof. C. A. Young on observations of the Transit of Mercury at Princeton, New Jersey. A Merz polarizing eyepiece was used, and the contacts were observed with powers 155 and 253, the probable error of the times recorded being less than one second. A flickering shade between the limbs was noticed at internal contact, both at ingress and egress, lasting for about 7^s at the former and 4^s at the latter. The planet was examined on the sun's disk with powers ranging from 100 to 500; but no bright ring nor bright spot was detected, though close round the planet a tendency to a radiating structure of the rice-grains on the Sun was noticed, as if the atmosphere of Mercury had an elongating effect, which would be exactly the opposite of the ordinary effect of refraction. This appearance was probably entirely subjective.

Mr. Ranyard also read a paper from Mr. Prince on an observation of the planet Mercury, made in the year 1867, at the time when snowflakes were observed by Capt. John Herschel in India passing across the field of the telescope. Mr. Prince noticed several seeds and a bright spot on the planet, the observation being made with a refractor, not a reflector, near the time of greatest elongation.

Mr. Christie read a paper on the question raised by Prof. Draper as to the existence of bright lines in the solar spectrum. He had examined with the Greenwich half-prism spectroscope the solar spectrum, and particularly the portion in the immediate neighbourhood of the G line on the less refrangible side, without being able to find any trace of bright lines. There were in particular two spaces which Prof. Draper considered to be bright lines corresponding to a double line of oxygen, and in his photograph the dark lines on each side appeared to be as wide as the bright spaces between and fuzzy at the edge, giving somewhat the character of bright lines; but as seen in the half-prism spectroscope, the dark lines were narrow and sharp, being not more than one-tenth of the width of the spaces between them, which latter appeared perfectly even in tint and quite sharp at the edges, the slit being, of course, very narrow. It seemed difficult to him to reconcile this with the theory that these spaces were really bright lines; for such lines when broad are invariably fuzzy at the edge. Moreover, in each of the spaces in question, two fine absorption-lines were detected, breaking up each of the supposed bright lines into three. This was shown by a diagram on the blackboard; and photographs were exhibited, taken with the half-prism spectroscope, in which the same fine lines were also distinctly shown.

The Astronomer Royal. I think I understood from you that the angular separation was greater than Prof. Draper seems to have used.

Mr. Christie. I am hardly able to speak upon that point, because I have only been able to compare this photograph of our eye observations with a print from one of his negatives; and it is hardly fair to compare the copy of a negative with the original. I should not like to say that the dispersion is very much greater; but certainly we can see a great deal more, and with a great deal more sharpness than is shown in the copy of Prof. Draper's photograph. As to the matter of the dispersion of the instrument, that depends upon how much the photograph was enlarged afterwards and upon the limit of enlargement which it will bear without a loss of definition. To enable a comparison to be made, I have brought some negatives taken at the principal focus of the Greenwich half-prism spectroscope without any enlargement whatever.

Mr. Ranyard. As to the question of the coincidence of the oxygen lines and the bright lines or interspaces in the solar spectrum, we cannot speak with absolute certainty with regard to any of these coincidences. We should be much nearer the truth if we said that with the particular dispersion used, they appear to coincide; or rather with the particular dispersion and with the amount of sharpness obtained we have not been able to see any difference between the places of the two series of lines; for whatever the accuracy and whatever the number of lines we may have corresponding with the dark lines of the solar spectrum, we are always open, on using an instrument of much larger dispersive power, to find that that which we did think was a coincidence was not really a coincidence. There was the celebrated case of the 1474 line, which we used to think coincided with a line of the iron spectrum, but which now is shown slightly to differ from it. We cannot be absolutely certain that the iron lines are coincident with dark lines in the Sun. On using a greater dispersive power we might find that there was some slight difference between the character of the two sets of lines. But what, I think, Prof. Draper has shown is this, that with the dispersion which he used, which was not a very high dispersion, there are some 11 lines of oxygen which appear to coincide pretty centrally with the interspaces or with bright lines in the solar spectrum, and that they appear also to coincide with them in relative brightness. At all events, that appears to be the case in his photographs; and if he had used a higher power with the oxygen spectrum it is possible that the dark lines might be found lying on the oxygen band. I think there is a question as to the great breadth of line not shading off; but to speak with certainty that would require a very sharp defining instrument, such as a "gitter" spectroscope, possibly defining more sharply than the spectroscope which Mr. Christie has been using. As to the fact of those fine lines being in the bright spaces, I do not see that that shows that these are mere interspaces. These may be lines of other elements overlying the spectrum of oxygen. We know that there are some elements which give bright lines. We know that the line D_3, which is seen on the chromosphere, is bright, and there is no dark line which corresponds with it. We know that there are great masses of gas, as there are in the nebulæ, which do give bright lines, that the gaseous matter which emits light to us does not absorb the light which comes from the inner part of the nebula, and the resultant light which comes to us gives bright lines. If there were gas in a similar position in the Sun we should expect to find that the light which was emitted from it did not blot out the continuous spectrum, but gave something in addition to it—that is to say, the brighter the spectrum the greater is the probability that if the Sun had the same oxygen as ourselves, there must be some oxygen there; and this would lead one to suppose that oxygen must be there, or if not, it must be in such an altered condition that it gives entirely different lines.

Mr. Schuster. I have not examined very much the spectrum of oxygen in the violet part, which is just the part under discussion. There are some bright lines of oxygen near F, and I have been carefully looking for them in the solar atmosphere, and they certainly seem to be neither dark nor bright. I may say that none of the lines of oxygen are sharp lines; so that a higher dispersion may possibly not be so good to detect the bright lines as a smaller dispersion; yet there is no doubt that you would expect a fading away of the bright lines from the middle towards the sides. I should like very much that Prof. Draper's discovery should be confirmed; but I candidly think that at present the probability is the other way. I sent a letter to 'Nature,' about last December, showing that oxygen gives at a lower temperature a different spectrum; and this spectrum, so far as I can judge, is reversed in the Sun; so that if Prof. Draper's discovery should be confirmed, I should be compelled to assume that the lower temperature spectrum is dark. But the lower temperature spectrum has only four lines. I could only find three with great accuracy, and they certainly seem to be reversed in the Sun. As to the theoretical question of bright lines in the Sun, I do not think the question is so simple as Mr. Ranyard has put it. In the nebulæ you have not got that very bright nucleus which the Sun has behind it. I should say, on the merely theoretical question, that it would be impossible to have bright lines in the solar spectrum, if you consider the Sun as a body at rest with different layers of gases, the hottest in the middle, and the temperature gradually decreasing as you go to the outside. No thickness of layer will produce a bright line then. The only assumption which can give you a bright line is the great disturbance in the body of the Sun which tends to throw out the hot gases from the middle of the Sun towards the outside, for you then have gas of a much higher temperature in contact with gases at a lower temperature; and then, I think, you can have bright lines, but not without.

Mr. Christie. May I be allowed to add a few words? I omitted to state one point with regard to the coincidence of the lines which I intended to mention. It seems to me that undue stress has been laid upon this fact. It is a known fact that none of the bright lines of oxygen coincide with the dark lines in the Sun. That is admitted, because they have none of them been identified; and therefore it follows, as a matter of course, that those bright lines of oxygen must coincide with spaces in the Sun. If the lines are as broad as the spaces, and we cannot find a coincidence within the limits of error of half the breadth of a space—and I should be inclined to take that as the limit of error in the photograph of Prof. Draper—those coincidences come to nothing at all, because then we should say that the bright lines of oxygen fall somewhere or other in these spaces; and they ought to coincide absolutely with the middle space. In fact, the definition in the photograph and the dispersion are not sufficient to determine that point. I am speaking entirely of the positive copy and not of the original negative; the definition in the original negative may be much better than that. So that the fact of 11 coincidences or 21 or any number of coincidences proves nothing whatever; because we know that there are no coincidences with the dark lines, and therefore there must be coincidences more or less perfect with the bright spaces. The whole question resolves itself into the accuracy with which you can determine those coincidences, which, as Mr. Ranyard has pointed out, is a question of dispersion and definition. As to definition, the test is the number of fine lines that can be seen; and I shall therefore content myself with saying, that in our half-prism spectroscope at Greenwich we can see and photograph 10 lines in the part of the spectrum under consideration where Ångström has only given 3. In that respect I think the definition is sufficiently good, at any rate, to show anything that could be shown in such a photograph as Prof. Draper's. I may say that more lines are shown on the board and in our photographs now shown than can be seen in Mr. Rutherfurd's photograph exhibited at the last meeting, which is, however, only a copy of the original negative. It is therefore hardly fair to compare it with our negatives; but, at any rate, in these latter the definition is very good. I may mention that I have examined the spectrum both with great dispersion and with great purity.

By reversing the spectroscope great purity in the spectrum is obtained; the lines are brought closer together, and you see these fine lines sharply defined, so that if there were any bright lines they would be brought out with great vividness. We are able to see the lines in the chromosphere of the Sun with great distinctness even when there is a considerable haze or cloud in the neighbourhood of the Sun; so that you would expect, if there were bright lines, to see some indication of them. There may be other ways of testing the question; but I must confess that I do not see my way to any other mode of looking at it at present. At the same time, I have only examined this part of the spectrum, because it was the part where the coincidences seemed most marked on Prof. H. Draper's photograph. With the large dispersion that I used I could not identify with certainty the spaces which Prof. Draper considered to be bright lines in other parts of the spectrum, on account of the number of fine absorption-lines brought out with the half-prism spectroscope.

Mr. Downing read a paper containing the proper motions of the stars in the first four hours of R.A. which are common to Bradley's Catalogue in Bessel's 'Fundamenta Astronomiæ' and the Greenwich Seven-Year Catalogue for 1864, the quantities of proper motion being deduced from a comparison of the places in these Catalogues. The places in the Seven-Year Catalogue have been corrected for the amount of proper motion applied to the observed place to reduce it to the mean epoch of the Catalogue; but the places in the 'Fundamenta' have been used without any correction whatever. Of course it is very easy to correct the resulting proper motions for any correction that may be applied to the 'Fundamenta,' as it is simply that correction divided by the number of years between the epochs of the Catalogues and applied with opposite sign.

The total number of stars whose proper motions are given in this paper is 176. There are some instances of considerable discordance between his results and the quantities given in the B.A.C. For example, the proper motion in R.A. of 32 *Cassiopeiæ* is, according to me, $-0^{s}\cdot002$, in the B.A.C. it is $-0^{s}\cdot050$; and to take another example close to this, he makes the proper motion in R.A. of 45 *Andromedæ* to be $-0^{s}\cdot003$, the B.A.C. gives $-0^{s}\cdot039$. And there are several other cases of as great, or nearly as great, a discordance.

Mr. Christie read a paper on the results which have been recently obtained at Greenwich for the motions of the stars in the line of sight, in continuation of two former papers upon the same subject communicated by the Astronomer Royal. Up to 1877, May 2, the ten-prism spectroscope was used, part of the train only being employed, giving a dispersion of two or four prisms; and from 1877, August 23, the half-prism spectroscope was brought into use; but the work was dropped until the middle of November, when systematic observations were again begun. With that spectroscope a dispersion equivalent to 4 or to 15 prisms of 60° was obtained according as one or two half-prisms were used, the latter dispersion being far greater than any which had ever been used before on stars. With that large dispersion in the case of the brighter stars, the lines could be measured with considerable accuracy; and even stars of the second magnitude have been observed with that power. On the whole, the results accorded very well with those obtained with the other spectroscope. A table was given showing the results obtained by Dr. Huggins, and at Greenwich with the ten-prism spectroscope, and also with the half-prism spectroscope; and it was worthy of mention that in the whole set there is only one case of discordance of sign. The motions of 51 stars in the line of sight have now been determined spectroscopically with more or less accuracy; and out of these there is only one discordance of sign, namely, in the case of the star β Leonis. The former results with the ten-prism spectroscope, depending only on a single night's observation, gave a motion opposite to that found by Dr. Huggins; but now, from a larger number of observations, the motion agrees with that given by Dr. Huggins, so that there is a complete confirmation of Dr. Huggins's results.

Mr. Gill. What was the extent of the change of sign? What proportion of the D interval would correspond with the measurement?

Mr. Christie. We have not put down the amount in the case of the ten-prism spectroscope, as there was only one observation. Dr. Huggins gives as his result simply +; but he did not feel certain of it: the result that we now obtain is +53, which is a large motion decidedly; but, of course, further observations may reduce this. But it does not do to place too much reliance upon the numerical results in all cases. In the case of some stars we are tolerably certain to a few miles, but in the case of some others you must expect an uncertainty of 10 or 20 miles. These observations are extremely difficult; and it is very easy to make a mistake of 10 or 20 miles in the mere uncertainty of bisection, so much depends upon the state of definition of the atmosphere.

Mr. Neison read a paper on some terms of long period in the mean motion of Mars. He stated that Leverrier's tables of Mars were 8″ out in the geocentric place of Mars during opposition, a very serious error. He had discovered some terms of long period in the mean motion of Mars, which would perhaps explain a portion of the errors in the place of Mars. Owing to the existence of these terms, Leverrier's value of the mean motion would be slightly too great; and this error in the mean motion, together with inequality in the motion of Mars due to these new terms, would partially explain the tabular errors in the place of Mars.

Prof. Cayley. And what do these terms arise from?

Mr. Neison. Some of them arise from the combined action of the Earth and Jupiter, others from the Earth and Saturn. As several have comparatively short periods, one of only 24 years, they would introduce systematic errors which could not be corrected by altering the mean motion. They would thus be more important.

Mr. Green. I have received a very interesting communication from Prof. Trouvelot and also from Dr. Terby on the subject of Mars. The result of Prof. Trouvelot's observations and drawings has been most satisfactory, and has confirmed in a most remarkable manner the majority of the drawings which have been made by me. I could even have matched them better from the drawings which I made in Madeira if I had made a selection with that object in view. In some instances the coincidence is most remarkable. I do think that we have escaped in a great measure the somewhat trite statement made in 1862 or 1864, that so many drawings had come before the Society that they were not able to harmonize them. It is very evident that Dr. Terby will be making a chart of Mars from all the drawings which have been sent to him. A communication from Professor Schiaparelli, of Milan, is very definite upon the same point. Prof. Trouvelot tells me he has made over 200 drawings of the planet Mars during the last opposition, and he feels himself thoroughly competent to make a map of the planet from them so far as the southern hemisphere is concerned. The question arises shall we on our part go without making a map, or shall we try to support the character of Astronomy in that respect here? I only wish most anxiously that the duty had fallen to Mr. Proctor, who has made a map of Mars previously, from which most of the continental observers have taken their names. He considers that it would be more fit in the hands of an observer than of one who merely uses the drawings of others. With the consent of the Society, I will endeavour to use the Madeira drawings for the production of a map of the southern hemisphere of Mars. It will necessitate the addition of a few names, because these markings, if named, are so much more interesting than if they are without names. Whenever any distinct marking is repeated again and again in several drawings, and there is no doubt at all about its existence, the name of the person who has made the most clear and definite drawing of it should be given to it. I think that would be one of the fairest things that we could possibly do. By that means we should spread a knowledge of the markings of the planet, and also add definite names to them. I will endeavour to do it with the utmost impartiality, and will promise to keep "Greenland" out of it.

The thanks of the Meeting were voted to Mr. Green.

Mr. Knobel exhibited one of the chronograph sheets of the Washington Observatory, showing the very perfect action of the chronograph for a run of two hours. A Kipps' conical pendulum was employed in this instrument.

The following papers were announced:—

Prof. T H. Safford. On the position of λ Ursæ Minoris.

Maxwell Hall. On the late opposition of Mars (extract from a letter to the Astronomer Royal).

Maxwell Hall. Observations of the Transit of Mercury 1878, May 6 (extract from a letter to the Astronomer Royal).

T. W. Backhouse. On the spectrum of the New Star in Cygnus.

Prof. W. Harkness. On the measurement of the inequality of the Pivots of Transit Instruments by means of the Spherometer-Caliper.

E. J. Stone. A reply to Mr. Proctor's note in the March number of the 'Monthly Notices.'

S. P. Langley. Observations of the Transit of Mercury made at the Allegheny Observatory.

S. W. Burnham. New Double Stars.

J. Tebbutt. Note on the Great Comet of 1861.

R. S. Newall. On the Atmosphere of Mars.

J. J. Waterston. On a Solar Thermometer Couple to measure the radiant force of daylight.

J. I. Plummer. Transit of Mercury, May 6, 1878.

Capt. W. Noble. Note on the Belgian Observations of the Transit of Mercury.

Lord Lindsay and Dr. Copeland. Observations of the Transit of Mercury, May 6, 1878, at Dun-Echt Observatory.

A. Marth. Ephemeris for finding the position of the Satellite of Neptune.

Dr. N. de Konkoly. The Transit of Mercury 1878, May 6.

The following were ballotted for and duly elected Fellows:—

Edouard G. Deville, Quebec; J. F. Main, B.A., University College, Bristol.

The Variable Star U *Cygni.*

THIS Star was observed by Mr. J. Birmingham on the 26th of May 1870 as a fine red star of the 8·5 magnitude. In October of that year I found it considerably fainter in magnitude, and further observation proved it to be variable.

I have now observed three maxima and three minima of the star; and combining these with two maxima and one minimum deduced from a discussion of Mr. Birmingham's magnitude estimates printed on pages 300 and 301 of his "Red Star Catalogue" ('Transactions of the Royal Irish Academy,' vol. xxvi. part vii.), I have obtained a pretty fair approximation to its elements, of which, so far as I am aware, no determination has as yet been published.

The following are the observed times of maxima and minima employed in the discussion:—

Observed times of maxima.	Observed magnitude.	Observer.
1871, June 16	8·1	Knott.
1872, September 16	7·9	Knott.
1873, December 29	7·7	Birmingham.
1876, July 18....................	8·0	Birmingham.
1877, October 21	7·8	Knott.

Observed times of minima.	Observed magnitude.	Observer.
1870, October 20	9·4	Knott.
1872, January 29	9·8	Knott.
1873, May 9	10·0	Birmingham.
1877, March 15	10·6	Knott.

From the five maxima we obtain ......Period=464^{d}·6, Epoch 1873, Dec. 28·7;
and from the four minima ....Period=467^{d}·9, Epoch 1873, May 11.

From these elements, the next maximum will occur on the 30th of January 1879, and the next minima on June 25th, 1878, and October 6th, 1879.

The elements represent very fairly the observations from which they are derived; but there appears to be some little irregularity both in the period and in the magnitude which the star attains at maximum and minimum. It would appear, for instance, from Mr. Birmingham's observations, that in the year 1875 the star passed a maximum about March 22, some sixteen days earlier than the date calculated from the elements; and the minimum of the same year also appears to have fallen some days earlier than the elements would predict. The observations are, however, hardly sufficient to fix the dates of the maximum and minimum of this year with precision, and I have therefore not employed them in my calculation of the elements. As there would seem thus to be some irregularity, and as there is a difference of 3·3 days between the period deduced from the maxima and that deduced from the minima, it may perhaps be convenient to assume 466 days as the approximate mean period, and the Epochs, for maximum 1873, December 29, and for minimum 1873, May 11.

The variable, which is of a fine red colour, is Zone +47° No. 3077 of the 'Bonner Sternverzeichniss,' its approximate place for 1880 being $\alpha=20^h\ 15^m\ 54^s, \delta=+47°\ 31'$. It is now (June 7) near minimum, of about $10\frac{3}{4}$ magnitude, and its red tint very marked. It has a bluish neighbour, 8·3 magnitude (which Mr. Birmingham also suspects to be slightly variable), at a little distance *n.f.*, with which, when near maximum, it contrasts in colour very finely. The star 32 Cygni precedes $4^m\ 10^s$, about 10′ further south.

George Knott.

Knowles Lodge, Cuckfield, Sussex,
1878, June 7th.

The Meteors of June 7.

On the evening of June 7 three very fine meteors were seen, two in daylight, at about 5.20 (I.) and 8.30 (II.) p.m., and the other at 9.50 p.m. (III.); and it is important to place the various accounts of them together for comparison. Many of these accounts are more or less imperfect, and some of them contradictory; but the fact of three fireballs appearing on the same date is in itself sufficient to justify the publication of the materials, crude and unsatisfactory as they are.

I. 5.15 to 5.20 p.m.—This meteor appeared in sunshine as a ball of fire descending very slowly towards the N.W. horizon. Several oral accounts have been given me by persons residing in this city. One observer states that his attention was directed to the sky by a bright object travelling along. At first he thought it must be a firework, but in its course it passed behind a cloud high above the W.N.W. horizon, and from this he inferred its celestial nature. At 8.30 p.m. he was surprised to see a similar object having the same direction, though the path was slightly below that of the earlier meteor (I). I went with him to the spot from which he saw the first meteor, taking my compass; and from his description it must have fallen to the N.W. (true) from a great altitude, south of the zenith. The second meteor, at 8.30, was a little below it; and his remark to a companion standing near him was, "There's another star going the same way." This simple expression is important as showing both meteors to have possibly had the same common radiant-point. The other observers could not be certain as to the direction, though the time was accurate.

II. The meteor at 8.30 p.m. was seen by Mr. A. Lighton, at Clifton, who describes its path from very high up N.N.E. to a point of disappearance at a considerable altitude above the horizon N.N.W., its descent being as nearly as possible at an angle of 30°. It moved very slowly.

Mr C. E. Cook, of Taunton, writes:—"It appeared to me to be almost overhead, moving from S.E. to N.W. or thereabouts, and unusually slow as it seemed below the hazy clouds with which the sky was covered."

A correspondent at Westbury on Trym writes that on June 7, at 8.20, the light of what afterwards proved a brilliant meteor caused him to look upwards. The body was travelling from east to west, and when first seen was almost exactly in the zenith, falling from thence towards the horizon. The nucleus was pear-shaped and of greenish hue.

Mr. W. Humphris, of Bath, saw this meteor. The sky was covered with cirro-stratus clouds at the time. The meteor took a direction from N.E. to S.W. or nearer W. He fancied he heard a crackling sound; the colour was that of the magnesium light; duration about 5 seconds. He did not see any tail, as daylight precluded the sight of it.

Mr. G. Holmes, Redland, Bristol, writes that the direction was from east to west, and it seemed to strike the earth on or near Durdham Down [N.W.]. Although seen in broad daylight the meteor was a singularly splendid object, appearing like a globe of liquid fire, with pale green tinge.

Another correspondent at Bristol describes the path as from south of zenith to a few degrees N. of N.W. (true).

Captain Cunninghame, of Bath, says that at 8.30 P.M. he saw a meteor of large size and bright blue colour in the heavens. Later on the same evening the Captain observed another and still more splendid meteor (III.) traversing the sky from east to west. Unlike the first it shot through the sky in a straight [horizontal] line, exhibiting the most brilliant green, purple, and gold rays. The nucleus was seen a few seconds after the tail had disappeared.

III. The meteor of 9.50 P.M. was also observed by many persons, including myself, at Bristol, where the sky was much overcast with cirrus cloud and haze (on which a bright lunar halo was afterwards formed), and this, combined with the strong twilight of a June evening, rendered the phenomenon less imposing than it might have been. The meteor had a very long path, almost horizontal, which it traversed E. to W. with a very slow motion, casting off a short train of sparks as it sailed along and showing sensible variations in the brightness of its pear-shaped nucleus. The observed portion of its path was noted from 1° above Spica Virginis to about 6° above the moon; but to include the whole extent of its visible course the line must be extended in each direction and have a length of perhaps 75°, from, say, slightly below α *Libræ* to slightly above α *Leonis*, running almost parallel with the ecliptic. The beginning and ending were not seen, however. Presumably the radiant-point was near *Antares*; and as the sky had become clearer between 11 and 12 P.M. a careful watch was directed towards the S.E. region, but no slow meteors were seen that could be associated with the fireball just described.

Mr. H. M. Rogers, writing to the 'Times,' from Knole Park (6 miles N. of Bristol), says:—"Last night [June 7] I was walking along a footpath close to Knole Park, my shadow falling on the park palings. Suddenly I saw two shadows, one of which was moving rapidly along the paling, the reverse way to the way I was walking. On turning round I saw the largest and brightest meteor it has ever fallen to my lot to witness. It was passing apparently from S. to N., very nearly parallel to the horizon, with a very slight declination towards the N. When I saw it first it was about 5° from the moon (taking the moon's diameter roughly at half a degree). It passed slowly along about 3° below the moon, or about 30° above the horizon, and continued its course for about 20° further towards the N., when it suddenly disappeared. The light was of a very pale green, as nearly as possible like the light of a glowworm highly intensified. As it passed under the moon its brilliancy caused the moon to look of a muddy yellow colour, like a street lamp in a November fog. The sky was fairly clear, but the stars were not shining brightly."

A correspondent at Cheltenham writes to the 'Times,' that in its slow majestic flight the meteor more resembled the passage of a Bengal light through the heavens than any thing else he could call to mind. Its path was almost due E. and W., and the apparent time of flight 20 seconds.

At Hawkhurst, Kent, an observer says:—"The moon was shining brightly in the W. and my attention was directed to a large and bright light moving slowly from S. to N., as nearly as I could judge in quite a horizontal direction at about 30° above the horizon. The meteor was in space about the size of a cricket-ball, and was moving through a hazy atmosphere, and surrounded by a smoky light which dimmed the brightness of the meteor. I was taken quite by surprise, and viewed it for a moment, and then began to count. I counted 34 not very fast [about 7 or 8 seconds?] before the meteor faded away. The time was 9.52 P.M. There was very little tail behind the meteor. The horizontal direction and slowness of the movement were very remarkable."

The Rev. J. Allen, Archdeacon of Salop, writing from Prees (14½ miles N. of Shrewsbury), says the meteor was seen by himself, by Sir E. Beckett Q.C., and others. It appeared first in the S. moving horizontally westward, slowly, like a railway train seen at a distance of near a mile. The colour was greenish white. At disappearance it broke into fragments of light. Duration 4 seconds or more.

A correspondent at Clifton describes it as a luminous body of tadpole shape and emerald-green colour, floating from S.W. to N.W., seemingly at no great distance above the houses. "Its velocity appeared to be slow, so much so that I involuntary ran about 15 yards to an opening between the houses, fully expecting to see more of this extraordinary apparition, but it had disappeared. I have seen many meteors, but never any thing so large, of such brilliant colour, and apparently so close to the earth."

Many other accounts of this meteor come from various parts of the kingdom.

Comparing the rough notes of these meteors the radiant-point would seem to have been low down in the S.E. near *Antares.* The long courses sufficiently indicate a centre on the horizon; but the first meteor can hardly have had the same focus, though the direction conforms well, for the radiant at *Antares* had not risen at the time of its apparition, and it may therefore have been directed from a shower further W., near α Libræ 223° −11° (G. and H. and Schmidt, May). The meteors II. and III. apparently converge on the same centre in Scorpio, not far from that of the detonating fireball of June 17, 1873, at 246° −20°, as calculated by Drs. Galle and von Niesel in Germany; and it is noteworthy that on the morning of June 7, 1872, the Italian observers recorded six shooting-stars with a focus fairly well-defined at 253° −24°. Further descriptions of the apparent paths of the three fireballs, on June 7 last, are required before the radiants can be fixed exactly, or their heights and velocities ascertained. The date is already stamped as noteworthy, and one on which future observations are especially desirable.

W. F. Denning.

Ashleydown, Bristol.

The Greenwich Visitation.

The Annual inspection of the Royal Observatory took place on June 1, when the Astronomer Royal presented his Report for the year ending 1878, May 2.

Under the head of Buildings and Grounds, a proposal is made to build a new library; and allusion is made to the new railway, which, though it passes within a distance of 1700 feet, has had no effect on the observations.

The instruments are maintained in good working order, the repairs required having been insignificant. A change is proposed in the arrangement for registering seconds from the Sidereal Standard Clock, in consequence of the variations in the rate caused by the contact springs, which are pressed together by a pin on the pendulum.

As regards the astronomical observations, the Moon, Sun, Planets, and important stars are observed with assiduity, the power of advantageous observation of small planets being, however, limited by the want of ephemerides. To facilitate the observations of stars a new Working Catalogue of more than 2500 stars has been prepared. With the Transit Circle 3970 transits and 3824 zenith distances have been observed. The moon has been observed 104 times with the Transit Circle, and 171 times with the Altazimuth.

Twenty-nine sketches of Mars have been made with the great Equatoreal, and several observations of the satellites of Mars and Saturn have been obtained. Six occultations of stars by the moon and seven phenomena of Jupiter's satellites have been observed. Clouds interfered with the observation of the Transit of Mercury, and no results of importance could be obtained, though ample preparations had been made.

The reductions are, as usual, in an advanced state, the transits being completely reduced at the date of the Report (May 2) to April 26, and the zenith distances to April 3. The computations for the Nine Year Catalogue of 2263 stars, including some supplementary investigations, were completed in the course of 1877, and the catalogue is printed as far as 16^h. For the determination of the systematic errors in Right Ascension depending on the assumed places of the clock-stars used, a comparsion has been made of the R.A. of clock-stars found from groups which extend over 12^h at least with those of the catalogue, and also a comparison of the R.A. of Circumpolar Stars above and below the Pole. The corrections range from $+0^s{\cdot}014$ about 23^h to $-0^s{\cdot}011$ about 9^h as depending on the R.A., and from $-0^s{\cdot}005$ at 50° to $+0^s{\cdot}003$ at 100° and upwards as depending on the N.P.D.

The R.A.'s of clock-stars in the first and second Seven-Year Catalogues have also been compared with the Standard 12^h groups, the result being to show that the errors of the assumed places of clock-stars, originally derived from Pond's observations, have been carried through the successive catalogues, though gradually diminishing in amount.

During the past year the Sun's chromosphere has been examined with the Spectroscope on 79 days (on 2 of these through part of the circumference only); prominences were seen on 58 days. The Sun was usually enveloped in mist or light cloud, when no prominences were seen, and it is therefore not safe to infer their complete absence on such occasions, as their brightness is generally much inferior to that of the chromosphere. All the observations, however, tend to show that the Solar prominences have been few in number and insignificant in size for many months. The Ten-prism Spectroscope was employed for this work up to the end of July last, when it was superseded by the new Half-prism Spectroscope, with the result of a marked increase in the proportion of days on which the chromosphere could be satisfactorily examined.

One hundred and thirteen measures have been made of the displacement of the F line in the spectra of 20 stars as compared with that of hydrogen; and 92 measures of the displacement of the *b* lines in the spectra of 16 stars as compared with that of magnesium, 21 observations of 6 of these stars having been made with the full dispersive power of the half-prism spectroscope (giving a spectrum of 75° from A to H, equivalent to 15 simple prisms of 60°).

Photographs of the Sun have been taken on 156 days, and 283 of these have been preserved for measurement. The photographs show a complete absence of spots on 66 days, and on 26 of these there are neither spots nor faculæ.

All observations with the spectroscope have been completely reduced, and the photographs have been completely measured to the end of 1877.

The correction of the position-angles and distances for the effects of refraction and distortion, and their conversion into Heliographic Longitude and Latitude, have been pushed forward as rapidly as circumstances would admit after the measurements had been completed. As there is a considerable accumulation of arrears since 1873, which will require many months for their reduction, it has seemed desirable to commence with the year 1876, with the view of including in the volume for 1876 the complete deductions from the measures of Sun-spots and faculæ in that year, if they can be prepared for press in time, leaving the complete results for the years 1873 to 1875 to be included in the next volume—the areas, as distinct from positions, having been already printed in the volumes for 1874 and 1875.

The daily dissemination through the country of accurate time signals, originating from this Observatoy, continues to advance without any important alteration of principle. There have been only two failures from accident in the automatic drop of the Greenwich Time-ball and 8 total and 25 partial failures in that of the ball at Deal. The Westminster clock has been 3^s in error on 7 days, and 2^s on 26 days; on the greater number of days the error was under 1^s. The number of chronometers being rated at the Royal Observatory is now 178.

In regard to the Transit of Venus Sir George Airy remarks:—

"At the date of the last Report, the determination of the longitudes of the British Stations was not quite complete (that of Kerguelen being then imperfect). But under a demand from the House of Commons, a strong effort was made to finish all introductory calculations, and to effect computations of Solar Parallax, by comparing all eye-observations of ingress of Venus among themselves, and all eye-observations of egress of Venus among themselves. The operation was in fact entirely conducted by Captain Tupman, though always under my cognizance. It soon became evident that the different stages of phenomena at the ingress must be most accurately discriminated; and this was done by Captain Tupman with great care, and I believe with great general success, although Captain Tupman himself has been induced lately to modify his interpretation of the observers' language in one or two instances. The same remarks apply to the recorded observations at egress. Then it became evident that in one instance there was indubitably an error of 1^m of time, and suspicions arose of a similar error in other observations. In an observation at another station, not included in the British 'five,' the more serious error of 30^s has been found.

"Finally, a Report was made to the Government on July 5, giving as the mean result for Mean Solar Parallax $8''\cdot76$, the results from ingress and from egress, however, differing to the extent of $0''\cdot11$. A more complete calculation by myself, including in one series the observations both at ingress and at egress, and recognizing the possible errors of R.A. and N.P.D., gave sensibly the same mean result for Parallax. This is liable to no error except from the interpretation of observers' language, to which I have alluded. All has subsequently been re-examined by Captain Tupman; different interpretations have, in a few instances, been put on the records; several observations from colonial stations have been combined; instead of using different phases in the observations (both of ingress and of egress), attempts have been made to ascertain the one phase of 'contact of limbs;' the notes of a few unpractised observers have been rejected; and the result for parallax has been increased to 8″·82 or 8″·83.

"The numerous photographs taken at the various stations had been carefully measured by Mr. Burton, and have since been re-measured by Captain Tupman; and (by photographs of Mr. De La Rue's scale of equal parts) the measure of photographic distortion had been well ascertained. But the results from photography have disappointed me much. The failure has arisen, perhaps, sometimes from irregularity of limb, or from atmospheric distortion, but more frequently from faintness and from want of clear definition. Many photographs, which to the eye appeared good, lost all strength and sharpness when placed under the measuring microscope. It was once remarked to me, 'You might as well try to measure the zodiacal light.' A final result 8″·17 was obtained from Mr. Burton's measures, and 8″·08 from Captain Tupman's."

Sir George Airy next alludes to the progress made in the Numerical Lunar Theory.

The developments of the effect of every possible error (expressed as a Symbolical Variation) in the coefficients and arguments of the assumed lunar ordinates, upon every term in the three fundamental expansions of—(1) Areas in the Ecliptic, (2) Radial Forces in the Ecliptic, (3) Forces normal to the Ecliptic,—have been computed and printed. The corresponding Solar Perturbing Forces have been computed entirely for the first of these (care being taken to extend the decimal calculation further for those terms whose effect may probably be increased in solution of the equations, a process in which many figures are almost necessarily wasted), and partially for the second and third. Until all have been completed, no positive inference can be drawn from the comparison of these terms with those of the ordinate-expansions; but a cursory collation of those relating to the Areas led to the supposition that there might be some error in the computations of the Annual Equation and related terms. A most jealous re-examination has, however, detected nothing, and has confirmed Sir George Airy's belief in the general accuracy of the numerical computations.

In conclusion, Sir George Airy urges the necessity for the erection of a new library, and, with this addition, believes that, if no remarkable increase be made in the number or magnitude of the instruments, the Observatory may go on for many years without requiring any extension of its boundary. He also suggests that the question of the extent of the printing should be taken into consideration by the Board, as the annual volume now contains very nearly one thousand large and closely printed pages, and every novelty in the Observatory increases the amount of printing. EDITOR.

CORRESPONDENCE.

To the Editor of 'The Observatory.'

Magnitudes of Meteors.

SIR,—

Mr. W. F. Denning's very interesting and exhaustive article on the average percentage of meteors, recorded in your last number of 'Observatory,' was perused by me with interest. Allow me to state that I believe my deductions approximately correct, owing to the fact that I paid particular attention to the magnitudes of those recorded and their velocities. There were many isolated meteors recorded by me of more than ordinary brightness; but I left them out of my deductions in every case, and only included those that were observed during some regular period of watching. I will further state, as a correction, that the observation of the August shower in 1871, published in the 'Astronomical Register,' as seen by Messrs. Tanger and Stephens, should have read Messrs. Sawyer and Stevens, the mistake in the names at the time never being corrected; this observation was included in my table.

I am, yours very respectfully,
EDWIN F. SAWYER.

36 School Street, Boston,
Mass., U. S. A., 1878, May 1.

Transit of Mercury.

SIR,—

As I find that clouds unfortunately prevented many observers from witnessing the ingress of Mercury in the recent Transit, I venture to send you my own observation, made with a 5½-inch achromatic. The sky became perfectly clear one minute before ingress commenced. Owing to my recent change of residence I have not been able to bring my instruments into perfect adjustment; I therefore only mention the times of contact which I noted as very roughly approximate.

At $3^h\ 11^m\ 47^s$ G.M.T. I first observed the planet suddenly forming a slight notch on the edge of the Sun. At $14^m\ 7^s$ I considered that geometrical contact appeared to exist. At $14^m\ 27^s$ the separation of the edges appeared complete, except that a narrow thin line in the direction of the centres of the disks was visible.

At $14^m\ 32^s$ a clear thread of light was evident between the planet and the apparent edge of the Sun. With powers 48 and 117 and the full aperture a bright ring slightly darker than the disk of the Sun appeared to surround the planet during the whole time that I watched it, viz. for about two hours, equal in breadth to about two thirds of the diameter of the planet (or rather less) and of uniform breadth. This was very evident to my assistant, a mechanic who observes well. For half an hour after ingress I saw a small spot of light (with both powers and 2 different dark glasses) a short distance from the centre of the planet in its south preceding quarter. I was not able to see it any longer, nor was it at any time very steadily visible, although the definition was very good. I then projected the disk of the Sun on a screen so as to be 6, 8, and 12 inches diameter. The planet only appeared as a round black spot with no halo or bright point. I saw no sign of the planet or of a bright ring round it outside the disk of the Sun.

I am, Sir, yours truly,
E. LEDGER.

Barham Rectory, Ipswich,
1878, June 6.

NOTES.

THE MEAN MOTION OF THE MOON*.—In a paper in the 'American Journal of Science' for Nov. 1877, Prof. Newcomb has made an important contribution to our knowledge of the motion of the Moon. In this paper appears the results of an elaborate investigation of the older observations of the place of the Moon, madé with the view of ascertaining how far the observed place of the Moon agreed with the theoretical place assigned to it by the Lunar Tables.

The data made use of in this investigation consist, first, of the Solar eclipses quoted by Ptolemy and those observed by the Arabians in the ninth and tenth centuries; and, secondly, of a number of occultations and eclipses observed by different astronomers during the seventeenth and early part of the eighteenth centuries. These last observations have hitherto been neglected, and the work of reducing them was found to be most laborious; but Prof. Newcomb is of opinion that the observations have proved to be well worthy of the pains which were taken with them. Unquestionably these observations will be invaluable if they be sufficiently numerous and exact to afford trustworthy corrections to the tabular places of the Moon, and Prof. Newcomb will have once more earned the gratitude of those engaged on the lunar theory. From the results he has obtained by reducing these last observations, Prof. Newcomb considers that we can trace the observed motion of the moon from 1750 back to 1675, and, with less exactitude, even to 1620—a most important extension.

Prof. Newcomb then uses the data obtained by this elaborate investigation to deduce the corrections indicated by observation to the tabular places of the Moon, employing Hansen's *Tables de la Lune* for the theoretical place. In this way Prof. Newcomb shows that although Hansen's Tables accurately represent the motion of the Moon during the interval 1750–1860, yet for epochs anterior to this period the tabular place of the moon deviates much from its observed place, the difference amounting to as much as 30″ during the latter half of the seventeenth century, a very considerable quantity.

Since 1860 the tabular place of the moon has been steadily gaining on its observed place, until at the present moment the former is fully 12″ in advance of the latter. This rapidly increasing discrepancy is a most serious matter, for it indicates the existence of some grave deficiency in the theory on which the tables are based. As these tables accurately represent the motion of the moon during the period 1750–1860, their sudden and rapid deviation from the truth appeared to be inexplicable and to point to the most alarming conclusions. Further inquiry has done much to modify this view. It is now generally known that this remarkable agreement between the observed and tabular places of the moon during the interval 1750–1860 is entirely due to an artifice made use of by Hansen. It arises from Hansen's having assigned empirical values to two terms of long period arising from the disturbing action of Venus. By this means Hansen was able to secure a satisfactory accord between his tables and observation for the interval 1750–1860, but at the sacrifice of his theoretical results. But unless these empirical values happened to agree very closely with the true values it was certain that before long their inaccuracy would become evident by a steady deviation of the tables from the truth. It is now known that at least one is certainly inaccurate, if not both. Later researches by Delaunay and Newcomb appear to show that whilst one of these empirical values is trustworthy, the other has only one-hundredth of the amount assigned to it by Hansen. Newcomb, therefore, points out that this term must be removed from Hansen's tables, and that they then present considerable errors for every epoch.

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* Amer. Journal of Science, 1877, Nov.

Prof. Newcomb proceeds then to inquire what corrections should be applied to Hansen's tables to make them more accurately represent the motion of the moon. He shows that not only must the mean motion of the moon be diminished by a small amount (29″ per century), but that the secular acceleration must be reduced from 12″·17 to 8″·40, thus bringing it much nearer to the theoretical value, which is 6″·18. Prof. Newcomb shows, however, that even when thus corrected Hansen's Tables fail to represent the position of the moon with the requisite accuracy, as there remain outstanding errors of very considerable magnitude, amounting, in fact, in 1875 to −15″·8, in 1775 to +12″·5, and in 1675 to −7″·4.

Prof. Newcomb then draws attention to the very singular fact that it is possible to entirely correct these large outstanding errors by merely changing the sign of the constant part added to the remaining term of long period due to Venus. It has been shown by Hansen, Delaunay, and Newcomb that this term has approximately the value of 15″·5 sin (A+30°); and Newcomb now draws attention to the fact that by writing it as 15″·5 sin (A−30°) the whole of the residual errors disappear. Yet Prof. Newcomb remarks that the former is indubitably correct and that it is merely a chance that these errors disappear in this manner.

In concluding this remarkable paper, Prof. Newcomb suggests that, as the case stands, the marked agreement between theory and observation which is produced by the introduction of this empirical term seems to be such as to warrant its provisional use until a more careful investigation of the subject can be made. E. N.

THE STAR-CLUSTER χ PERSEI*.—Dr. Vogel, of the Potsdam Observatory, has published the results of measures of the cluster χ Persei made in 1867–70, by means of the 8-inch refractor, at Leipzig, with the object of fixing the relative positions (and magnitudes) of the stars of this cluster, so that any future change may not pass undetected. 176 stars, in all, have been fixed in position by the filar micrometer. The field was bright and a magnifying-power of 145 diameters was employed throughout. The various sections of this work of 36 quarto pages treat of the following subjects:—

§ 1. *The position of the instrument—the determination of the parallel.* A novel feature here is that the zero of position was determined before and after each set of measures (either of p and s or of $\Delta\alpha$ and $\Delta\delta$) on each pair of stars, so that the observations for any pair are arranged thus:—1st, four measures of p, four measures of s: 2nd, determination of parallel: the next pair is then observed as follows—1st, four measures of p, four measures of s; 2nd, determination of parallel: and so on throughout the night.

§ 2 contains an *investigation of the position-circle*, and of the *value of the revolution of the micrometer*. The zero of the micrometer was found to be dependent upon the position of the instrument, and also upon the kind of illumination of the threads. The value of the revolution is found from transits on 20 nights, from November 1867 to May 1870. During all this time the reticule was left at the same distance from the objective, and the thermometric coefficient resulted $+0''\cdot001581\ t^{0}$ in Réaumur's scale. The magnitude and the *sign* of this, Dr. Vogel explains by the fact that the focal point was not determined each night (as, he says, is usual), and he correctly points out the necessity of leaving the focus unchanged for such observation and for determining the value of the screw during the series itself.

§ 3 deals with *the methods of observation and reduction.* The brighter stars less than tenth magnitude were determined from measures of p and s with four selected stars of the group. These four were connected by measures of p and s, and also $\Delta\alpha$ and $\Delta\delta$; and they were further connected with two stars of the cluster *h Persei*, which had been observed with the Bonn meridian-circle. The pairs of stars were observed alternately on different threads, the zeros determined under the same circumstances as those of the observations, and the position-angles measured by turning the circle in both ways. From four to six measures of p and s were made each night, and for each pair of the brighter stars at least four nights' observations were made, *i. e.* at least sixteen measures of p and s. The reductions are complete and the observations are reduced to 1870·0.

* Der Sternhaufen χ Persei von Dr. H. C. Vogel: Leipzig, 1878.

The fainter stars (10–12 mag.) were observed by $\Delta\alpha$ and $\Delta\delta$, with other stars, at least on two nights for each star.

§ 4 deals with the *accuracy of the observations*; for the brighter stars the probable error of a *single observation* is found to be, in *s* $\pm 0''{\cdot}228$, in *p* (reduced) $\pm 0''{\cdot}306$. The probable errors for the *mean of each night* are more important, and result as follows (no dependence of the probable errors on the distance or position-angles being evident):—Probable error of one night, in *s* $\pm 0''{\cdot}190$, in *p* (reduced) $\pm 0''{\cdot}165$. For the final position (at least four nights) these become, in *s* $\pm 0''{\cdot}092$, in *p* $\pm 0''{\cdot}080$. For the $\Delta\alpha$ and $\Delta\delta$ of the brighter stars these are $\pm 0''{\cdot}097$ in R.A. and $\pm 0''{\cdot}089$ in N.P.D. The positions for the fainter stars are determined within less than 1″ in each coordinate, which Dr. Vogel considers sufficient for his purpose.

§ 5 treats of the *determinations of the brightness of the stars of this cluster.* The 176 stars of the cluster range between the 6·5 and 13 magnitudes. Each one of the fainter stars (higher than 10 mag.) was determined by eye estimates of magnitude at least five times; the probable error of the mean is $\pm 0{\cdot}14$ magnitude. The brighter stars were determined on several evenings by the eye, and on two nights each was compared by a Zöllner's photometer with one of the standard stars. A table gives the magnitudes of the brighter stars:—1, by eye (Vogel); 2, by eye (Argelander); 3, photometric magnitude, assuming the light ratio $\frac{1}{2{\cdot}519}$ or 0·397. The agreement is remarkable, but the table shows (what was already known) that Argelander's magnitudes higher than $9^{m}{\cdot}0$ make the stars too bright.

§ 6 gives the *observations of the stars* (in tabular form) *and the results.* A difference between the spring and autumn observations in *both* $\Delta\alpha$ and $\Delta\delta$ of one of the stars indicates possibly a parallax of $0''{\cdot}3$.

§ 7 gives the *observations of the fundamental stars, and catalogue of the 30 brightest stars.* The observations are of relative $\Delta\alpha$ and $\Delta\delta$ of the four fundamental stars, and of two of Argelander's stars in *h Persei* and also meridian observations. These last also indicate a parallax to the star *b*. None of the stars appear to have a large proper motion.

§ 8 deals in the same way with *observations of the fainter stars, and catalogue of all the stars of the cluster.* This is followed by two charts, one of the brighter stars and the plan of triangulation, the other of the whole cluster.

This brief analysis will give an idea of the contents of this extremely thorough paper, which will take its place beside the other researches of the author in the same field. They are all models of what such investigations should be, and leave nothing to be desired, in methods of observation or reduction, in the accuracy of the final results reached (which are always adequate to the purpose in hand), and, finally, are excellent examples of the literary style and clearness appropriate to such memoirs.—*Amer. Journ. Sci.*

Astronomical Memoranda for 1878, *July* 15–*August* 14.

July.	G. M. T.	Phenomena.
	h m	
15. M.	9 46	♃ 3 Sat. Oc. R.
	14 28	θ Capr. Oc.† 329°
16. Tu.	14 12	♃ 1 Sat. Sh. I.
	14 24	♃ 1 Sat. Tr. I.
17. W.	11 31·4	♃ 1 Sat. Ec. D.
	14 0	♃ 1 Sat. Oc. R.
18. Th.	11 0	♃ 1 Sat. Sh. E.
	11 10	♃ 1 Sat. Tr. E.
	11 59·7	♃ 2 Sat. Ec. D.
	8 20	♃ rises.
	10 28	♄ rises.
	12 35·3	♃ tr. D. 20° 6′ S.
	13 32	♀ rises.
	16 7	☉ rises.
	16 12·7	♈ transits.
	16 27·0	♄ tr. D. 1° 1′ S.
	21 36·8	♀ tr. D. 21 48 N.
19. F.	0 6·0	☉ tr. D. 20 51 N.
	1 13·1	☿ tr. D. 18 38 N.
	8 5	☉ sets. No real night.
	22	☾ Apogee.
20. S.	9 48	♃ 2 Sat. Sh. E.
	10 2	♃ 2 Sat. Tr. E.
21. S.	14 29	♃ 4 Sat. Sh. I.
		☿ ☌ ♂ ☿ 0° 19′ N.
22. M.	10	
☾	0 15·9	
	13 3	♃ 3 Sat. Oc. R.
23 Tu.	21 42·4	♀ tr. D. 22° 16′ N.
24. W.	1 27·3	☿ tr. D. 15 36 N.
	13 24	♃ 1 Sat. Oc. D.
	21	♃ ☍ ☉
25. Th.	10 34	♃ 1 Sat. Tr. I.
	10 35	♃ 1 Sat. Sh. I.
	12 54	♃ 1 Sat. Tr. E.
	12 54	♃ 1 Sat. Sh. E.
	14 29	♃ 2 Sat. Oc. D.
26. F.	10 10·5	♃ 1 Sat. Ec. R.
27. S.	9 21	♃ 2 Sat. Tr. I.
	9 29	♃ 2 Sat. Sh. I.
	12 18	♃ 2 Sat. Tr. E.
	12 25	♃ 2 Sat. Sh. E.
28. S.	11 50·7	♃ tr. D. 20° 25′ S.
	15 33·4	♈ transits.
	15 47·2	♄ tr. D. 1 7 S.
	21 48·4	♀ tr. D. 22 28 N.
	21	♀ ☌ μ Gem. * 7′ N.
29. M.	0 6·2	☉ tr. D. 18° 45′ N.
	1 37·5	☿ tr. D. 12 24 N.
July.	G. M. T.	Phenomena.
	h m	
●	9 40·3	Eclipse of Sun.
	12 41	♃ 3 Sat. Oc. D.
31. W.	19	☾ Perigee.
Aug.		
1. Th.	12 17	♃ 1 Sat. Tr. I.
	12 29	♃ 1 Sat. Sh. I.
2. F.	9 34	♃ 1 Sat. Oc. D.
	12 5·4	♃ 1 Sat. Ec. R.
	21 54·5	♀ tr. D. 22° 25′ N.
3. S.	1 43·9	☿ tr. D. 9 9 N.
	9 3	♃ 1 Sat. Tr. E.
	9 18	♃ 1 Sat. Sh. E.
	11 37	♃ 2 Sat. Tr. I.
	12 7	♃ 2 Sat. Sh. I.
5. M.	9 17·2	♃ 2 Sat. Ec. R.
☽	1 19·2	
6. Tu.	7 34	4 Scorp. Oc. D. 40°
7. W.	6 55	♃ rises.
	8 36	♃ 4 Sat. Sh. I.
	9 9	♄ rises.
	10 6	♃ 4 Sat. Tr. E.
	11 6·2	♃ tr. D. 20° 42′ S.
	13 11	♃ 4 Sat. Sh. E.
	13 54	♀ rises.
	14 54·1	♈ transits.
	15 6·8	♄ tr. D. 1° 17′ S.
	16 36	☉ rises.
	22 0·6	♀ tr. D. 22 6 N.
8. Th.	0 5·4	☉ tr. D. 16 8 N.
	1 46·7	☿ tr. D. 6 1 N.
	7 35	☉ sets.
	7 49	6063 Oc. D. 62°.
	8 21	☿ sets.
	10 10	Twilight ends.
9. F.	8 40	σ Sagitt. Oc.† 350°
	9 14	♃ 3 Sat. Tr. E.
	10 48	♃ 3 Sat Sh. E.
	11 19	♃ 1 Sat. Oc. D.
10. S.	8 52	♃ 1 Sat. Sh. I.
	10 47	♃ 1 Sat. Tr. E.
	11 12	♃ 1 Sat. Sh. E.
11. S.	8 29·1	♃ 1 Sat. Ec. R.
12. M.	11 52·7	♃ 2 Sat. Ec. R.
○	12 16·1	Eclipse of Moon.
	17	☿ Aphelion.
	22 6·6	♀ tr. D. 21° 31′ N.
13. Tu.	1 45·6	☿ tr. D. 3 7 N.
	10	☿ elong. 27 23 E.

† A near approach.

EDITOR.

Meteor Notes for July.

This is a favourable month for meteor observations, though the prevailing twilight and short nights prevent much work of this kind. During the first 11 days morning watches are advisable, and, after about the 22nd, observations should be resumed in the evenings. In July the visible number of shooting-stars is often considerable; they proceed from a vast number of different radiants. Observations are chiefly required of the N.E. and N.W. skies, in which there must be many unknown systems. Two of these, slightly observed last year in Perseus at 36° +47°, and 47° +45°, should be looked for. That rich August shower—the *Perseids*—begins in July, and more observations are required to fix the date with certainty. These meteors are rarely without streaks, and such only as have this distinguishing feature can be fairly called *Perseids*.

July furnishes us with many radiants in Andromeda, Lacerta, Pegasus, Cygnus, &c., and there is a shower at 12° +70° (G. and H.) probably distinct from the July *Cassiopeids*, also at 293° +42° near δ Cygni, different from the August *Lyrids* at 285° +44° (G. and H.). A fine shower of bright swift meteors leaving streaks was traced last year from 6° +53°, confirming S. & Z. 7° +50° (July). On the 20th there was a strong shower from 317° −11°, being the same as one previously discovered by Tupman at 313° −6° on July 21–22. On the 16th, in 1871, a well-marked shower was noted by Prof. Herschel from 257° +36°, and it has been verified by several more recent observations. Radiant-centres at 64° +46°, 250° +62°, and 349° −1° require further investigation; and it will be the chief work of future observers to single out and separate these and other systems from closely bordering showers and fix their positions and periods exactly. Many interesting observations have been rendered useless by reason of their scanty nature. An observer watching for say two hours only will see just enough to indicate a series of uncertain showers, giving perhaps two or three meteors each, whereas if his observations are continued for five or six hours he will gather sufficient paths to render them all certain. Amateurs cannot, however, be expected to carry on such lengthy and trying work; but there is no doubt as to the importance of long watches as giving plentiful and accurate results.

Recent Observations.—Mr. H. Corder sends a list of radiants found by him in March, April, May. Between March 10 and April 3, a shower of quick meteors with streaks was seen from 250° +54° (12 /'s), which confirms one observed by the writer in 1877, March 14–April 19, at 250° +51° (15). The *Lyrids* were noted on April 20, P.M., with radiant at 275° +35° (11) as last year. Another active shower was traced in April–May from 174° +7° (19). On May 4 A.M., 3 of Tupman's *Aquarids* were observed; they indicated the radiant at 334° −1° and were remarkable for great length of path. Positions at 300° +20° (7), and 145° +77° (7), which were also determined (for April–May), confirm two showers recently deduced for April 19–23 at 302° +18° and 130° +79° (see *Monthly Notices*, vol. xxxviii. p. 396). The *Coronids* were slightly seen with a centre at 242° +27° (8). W. F. Denning.

Position of the Sun's North Pole.

1878.	Pos.-angle.	Dist. from limb.
	° ′	° ′
July 19	5 39	+4 54
24	7 49	5 20
29	9 54	5 43
Aug. 3	11 55	6 4
8	13 49	6 23
13	15 38	+6 39

A total Eclipse of the Sun, visible in N. America, occurs on July 29. The central line passes near Denver.

The Moon.

1878.	Rises.	Geoc. Libr. at midnight.		1878.	Sets.	Geoc. Libr. at midnight.	
		long.	lat.			long.	lat.
	h m	°	°		h m	°	°
July 16...	9 11	+3·19	−1·20	Aug. 1...	8 41	+0·46	+4·25
17...	9 24	2·11	2·58	2...	8 53	1·90	5·46
18...	9 35	+0·88	3·83	3...	9 9	3·21	6·31
19...	9 46	−0·42	4·92	4...	9 27	4·32	6·75
20...	9 59	1·73	5·77	5...	9 51	5·18	6·79
21...	10 11	3·00	6·39	6...	10 22	5·76	6·43
22...	10 26	4·12	6·74	7...	11 7	6·06	5·72
23...	10 47	5·04	6·79	8...	12 5	6·07	4·72
24...	11 16	5·68	6·52	9...	13 14	5·83	3·50
25...	11 54	6·00	5·93	10...	14 29	5·34	2·12
26...	12 49	5·97	5·01	11...		4·63	+0·66
27...	13 59	5·57	3·78	12...	Full.	3·73	−0·80
28...		4·81	2·30	13...		2·67	2·21
29...	New.	3·76	−0·64	14...	Rises.	1·49	3·52
30...		2·47	+1·08	15...	7 54	+0·21	4·64
31...		−1·03	+2·76	16...	8 7	−1·10	−5·56

A partial Eclipse of the Moon, visible at Greenwich, occurs on Aug. 12. Magnitude of the Eclipse 0·590.

First contact with Penumbra $9^{h}\ 22^{m}{\cdot}5$; with shadow $10^{h}\ 42^{m}{\cdot}6$.
Last „ „ $14^{h}\ 53^{m}{\cdot}9$; „ $13^{h}\ 33^{m}{\cdot}8$.

A. M.

The Planets.

Mercury is an evening star at greatest elongation east (27° 23′) on August 13. He sets 56^{m} after the Sun on July 23 and 41^{m} after on Aug. 13, when he is so far south of the Sun that much of the advantage of the great elongation is lost.

Venus is a morning star, rising between 13^{h} and 14^{h}. Her disk is gibbous, the illuminated portion on July 15 being 0·790, and on August 15 0·873, and the corresponding diameters 13″·2 and 11″·6.

Jupiter is in opposition on July 24, retrograding through a small arc about 5° south of α and β Capricorni.

Saturn is a morning star, stationary, about midway between β Ceti and γ Pegasi. The following are the dimensions of the ring:—

	Outer Ring.		Inner Ring.		Ball.
	Maj. Axis.	Min. Axis.	Maj. Axis.	Min. Axis.	Diam.
July 24	41″·55	3″·52	27″·63	2″·34	16″·6
Aug. 13	42″·77	3″·35	28″·44	2″·23	17″·2

The south side is visible, the elevation of the Earth above the plane of the rings being 4° 52′ S. and 4° 30′ S. on July 24 and August 13 respectively, and of the Sun 2° 30′ and 2° 48′ S. EDITOR.

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Elementary Arithmetic and the Duration of Totality in a Solar Eclipse.

In elementary works upon Astronomy we often meet with numerical statements which are read with little interest, because no indication is given of the methods by which they are obtained. The unscientific reader is also frequently puzzled by the discrepancies which he meets with in such statements. Very probably, if he endeavours to seek for any further explanation, the only answer which he obtains to his inquiries is, that the numerical results given in various books differ, because different values of the constants used in the calculations have been employed, the progress of observation or theory having from time to time shown that such values needed more or less alteration. Or, it may be, that he is told that some of the results stated are derived from methods involving only a rough approximation to accuracy; others from more intricate and carefully developed methods. He may even be informed that one result is correct to the first order of small quantities, and another to the fifth order. If a reader is not mathematically educated he probably feels no wiser, and possibly only more confused as the result of his inquiries.

It has therefore occurred to me, in endeavouring to render lectures upon Astronomy interesting to a popular audience, that it might be well to work out on a black-board, in an extremely rough manner, and without any attempt to assume more than an acquaintance with some of the most ordinary rules of Arithmetic, a few of those numerical statements which are often met with in elementary manuals, but seldom, if ever, elucidated in them.

I proceed to give an example or two of the results obtained by these rough methods, which may, I think, be interesting to some of the readers of 'The Observatory.' They may also suggest that, in the teaching of arithmetic in schools, a large number of numerical questions may easily be composed by an intelligent teacher, of such a nature as may illustrate astronomical facts, and relieve the weariness of simple numerical computation, by giving an opportunity for a brief explanation of the phenomena involved.

An instance of such a question, in which the figures are arranged to give a simple numerical result (14 seconds), but are yet sufficiently near the truth to form a foundation for instruction, is to be found in the Elementary Arithmetic paper of the Cambridge University Local Examinations of December 1876,—"How much less time will it take for light to pass from the Sun to Jupiter, if it travel 186,000 miles per second, than if it travel 185,000 miles per second, given that the earth is 91,760,000 miles from the Sun, and Jupiter 5¼ times as far?"

One interesting fact may be illustrated as follows:—The Moon, when overhead, is about 4000 miles nearer to an observer than when on the horizon, or than it would be if seen from the Earth's centre. Now 4000 miles is about $\frac{1}{60}$th of the distance of the Moon from the Earth's centre; therefore, when overhead, it must appear to be about $\frac{1}{50}$th part broader in its diameter. Taking its maximum apparent diameter as 33½ minutes, or 1996 seconds of angular measure, the $\frac{1}{50}$th part of this is about 40 seconds.

Now it is stated in some elementary books that the totality of the great total Eclipse of the Sun on August 17, 1868, was considerably lengthened in duration in those regions (*e.g.* the Gulf of Siam and parts of India) where its duration was longest, by the fact that the Sun and Moon were at the time nearly vertically above an observer; the duration in the Gulf of Siam being, I believe, about 6 minutes 50 seconds, and the Sun only about 2½° from the zenith.

Let us see how the figures above referred to in connexion with the Moon's apparent size will simply elucidate this. From the value (40″ of angular measure) obtained above for the apparent increase of the breadth of the Moon when seen in the zenith (which of course is contrary to the popular notion and the optical delusion of its apparently greater size when seen on the horizon), it will follow that the totality of a Solar Eclipse may be increased by ⅓rd part of what would otherwise be its maximum duration, by the effect of this cause. This may be shown as follows:—

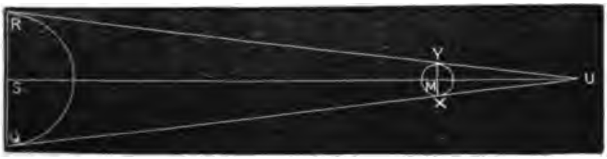
Independently of other causes, such as the situation of the place of observation being near to the equator, so as to cause the rotation of the Earth as far as possible to prolong the duration of the eclipse, a total eclipse will last longest when the Sun appears to have its minimum and the Moon its maximum diameter. The maximum diameter of the Moon, as seen from the centre of the Earth, we may take, as before, to be 33½′ of angular measure. The minimum value of that of the Sun we may roughly take to be 31½′. The difference is 2 minutes, and consequently totality would last, as seen from the centre of the Earth, during the time that the Moon would take, relatively to the Sun, to make the extra 2′ of angular measure in its breadth pass between us and the Sun.

Wherever on the Earth's surface the place of observation may be, the Sun will appear appreciably of the same size as if seen from the

Earth's centre; and if the Moon be near the horizon, the Moon will appear to be very nearly of the same size as if so seen. In this case the eclipse will last for the time above explained. But, as we previously showed, if the Moon be vertically overhead, it will appear to be 40″ of angular measure broader than if seen from the Earth's centre; and the eclipse will last, not simply while the extra 2′ of the Moon's apparent breadth passes between us and the Sun, but while 40″ of additional apparent breadth also so passes. And 40″ being one-third of 2′ (or 120″), we see, in this elementary manner, that the maximum duration of a total Solar Eclipse, so far as it depends on the apparent diameters of the Sun and Moon, may be increased by one-third of what would otherwise be its duration, by the fact of the Moon being at the time vertically over the observer, and in consequence appearing to be larger than when seen on the horizon. I have not seen this result thus worked out in any elementary manual, and I think the proportionate increase of duration, which is so great as to have a most important effect on the interest and phenomena of any eclipse affected by it, is longer than a popular audience would be prepared to expect.

Another illustration of such rough but interesting calculations may be found in connexion with the breadth of the shadow of the Moon on the Earth at the time of a Total Solar Eclipse. Hardly any two books give the same value. Hardly any explain how it may be approximately calculated. We read in that very excellent elementary treatise 'Astronomy without Mathematics' (5th edition, p. 142), that under the most favourable circumstances, such as have been referred to above, when the Moon is at its nearest to the Earth, so as to look its largest, and the Sun at its furthest, so as to look its smallest, the zone of shadow cannot exceed in breadth 148 miles. In Brinkley's 'Astronomy,' recently edited by Stubbs and Brünnow, 166 miles is given as the maximum value. In several books 180 miles is, I believe, given. In the article upon Astronomy in the new edition of the 'Encyclopædia Metropolitana,' we find 173 miles. I know not whence or how this last result is obtained, although I presume it is the most accurate known; indeed I have never met with any attempt to work out any of the above values numerically.

We may, however, roughly work the problem out as follows:—

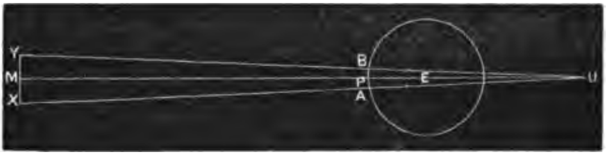


If we take the Sun at its *greatest* distance from the Earth, and let Q S R be its diameter of about 850,000 miles, the distance from S, its centre, to the centre of the Earth may roughly be taken as 93,000,000 miles.

If X M Y be the diameter of the Moon, of 2160 miles, the least distance of its centre, M, from the Earth's centre may be taken as 221,600 miles. Consequently under such circumstances we may take S M to be about 92,778,400 miles, the difference of the two above-named distances. If U show the point beyond the Moon to which its shadow, which will produce a total eclipse, will extend, S U will evidently be as many times greater than M U as Q R is greater than X Y. But 850,000 miles, the value of Q R, is about 393 times 2160 miles, the value of X Y.

Hence S U is about 393 times M U, and S M will be 392 times M U; whence, dividing 92,778,400 (the value of S M) by 392, we get for the value of M U about 236,937 miles, or say 237,000 miles. This will be the greatest distance to which, under the most favourable circumstances, the extreme point of the shadow of the Moon, which will produce a total eclipse, will extend beyond the Moon's centre.

Let us see now how large a breadth of the Earth's surface this will cover; and let us for simplicity take the centres of the Earth, Sun, and Moon to be in the same straight line.



Let M, as before, be the centre of the Moon, and let the shadow Y U X meet the Earth, so that B P A is its width. Then M U equals, as we have calculated, about 237,000 miles; and M E being equal (as we took it) to 221,600 miles, E U will be 15,400 miles, But P E, the radius of the Earth, being roughly 4000 miles, we shall have P U equal to about 19,400 miles, or say 19,000 miles, inasmuch as one or two points in our calculation have tended to give us rather more than the true value. B A will then, if treated roughly as a straight line, evidently be the same fraction of X Y, the diameter of the Moon, as P U is of M U; that is, as 19,000 is of 237,000. Wherefore B A will equal $\frac{19}{237}$ of X Y, or $\frac{19}{237}$ of 2160 miles, which is equal to about 172 miles.

I think that this will be found to be a very fair approximation to the maximum possible breadth of the band of shadow traced across the Earth's surface by the umbra thrown upon it from the Moon in a total Solar Eclipse. It certainly agrees in a remarkable manner with the value given in the article already referred to in the 'Encyclopædia Metropolitana.' It is easy to work out a result, of about 216 miles, in a similar manner for the maximum breadth of the zone within which a Solar Eclipse may be annular. It will, I trust, be understood that I am fully aware of the almost absurdly rough and elementary manner in which this calculation has been made. Still I hope it may show how, without more than the use of a little proportion and a few fractions, boys and girls in schools, or a popular audience of unmathematical folk, may be enabled to go a little deeper into some appreciation of the numerical statements made in ordinary elementary treatises than they are in general invited to do.

It is also interesting to notice that it may easily be shown by similar elementary and rough calculations (with which, however, I will not here trouble the readers of 'The Observatory'), that such a shadow would, owing to the combined motions of the Earth and Moon in their orbits, pass over the point P at the rate of about 37 miles per minute. For a shadow 173 miles wide, this would give $4\frac{25}{37}$, or very nearly $4\frac{2}{3}$ minutes; *i. e.* 4 minutes 40 seconds. This duration may be shown to agree well with that obtained by another simple method, provided we are careful to take account of the difference in the apparent size of the Moon, as seen vertically overhead, or from the centre of the Earth, to which we have already referred.

This has been impressed upon me by the following circumstance. I remember being momentarily puzzled in teaching a class by a calculation made as follows. The Sun's minimum apparent diameter being 31½ minutes of angular measure, and the Moon's maximum apparent diameter 33½ minutes, or 120 seconds more, and the Moon at such a time passing over about 34 seconds of angular measure in a minute of time, it seemed that the Moon would take about $\frac{120}{34}$ minutes of time, or about 3 minutes 30 seconds, to pass over the above difference; or, that about 3 minutes 30 seconds would be the longest possible duration of the totality of a Solar Eclipse (neglecting the rotation of the Earth). This appeared to contradict the previously obtained duration of 4 minutes 40 seconds. But upon noticing (as was shown at the commencement of this article) that the maximum duration of totality is lengthened by one-third owing to the greater apparent size of the Moon when seen vertically over any place, compared with the size it would appear to have from the centre of the Earth, and increasing the 3 minutes 30 seconds by ⅓rd part, and so bringing it to 4 minutes 40 seconds, the two results exactly corresponded.

The maximum possible duration of totality, when the rotation of the Earth is taken into account, may be deduced from the above calculations as follows:—We have assumed the shadow to be sweeping directly across the place P at the rate of 37 miles per minute. If P be on the equator of the Earth it will be revolving in the same direction at the rate of about 25,000 miles in 24 hours, or about 17 miles per minute, and the shadow will therefore pass over the place P at about 20 miles per minute only, instead of at 37 miles per minute. Taking the breadth of the shadow, as before, to be 173 miles, this would make the maximum possible duration of totality about 8½ minutes—a result which is really rather too great. The great Eclipse of August 17, 1868, was total for nearly 7 minutes in the most favourable places for observation, and is, I believe, the longest ever recorded. But even in that remarkable instance the extreme limits of duration were not reached. The exact calculation of the maximum possible duration under an imaginary coincidence of every most favourable condition is, of course, highly complicated. The most recent statement of the exact value for a place on the equator which I have met with is one made by Du Séjour, in the year 1777, to the French Academy of Sciences. His result was 7 minutes 58 seconds. This value might, doubtless, be somewhat modified by the use of the most accurate values of elements now known. It is, however, not very different from the rough arithmetical calculation of 8¼ minutes made in this article, and, I think, sufficiently near to it to show that such rough calculations may help to make astronomy interesting to those who cannot use higher mathematical processes. I have chosen this example of the application of simple arithmetic to the calculation of the maximum possible totality of a Solar Eclipse, not because it is one of the easiest examples of such calculations that I might have chosen, but because it is one that has recently come before me in my lectures, and one which may suggest, from the somewhat difficult nature of the problem involved, that many other and simpler arithmetical illustrations of elementary astronomical principles may easily be found.

E. LEDGER.

Binary Stars.

It is well known that Sir W. Herschel was the first who proved the existence of Binary Stars. He did this by the aid of micrometric measures which he had made with the view of finding the parallax of fixed stars, and since that time the two subjects have been closely allied*. Astronomers have, at the same time, investigated both, and with about the same kind of apparatus as Herschel, which, in the course of time, has been highly improved. Before that the public opinion concerning the existence of Binary Systems was divided. Lambert and other astronomers were inclined to doubt it, while John Mitchell and others considered a physical connexion between fixed stars highly probable, and Christian Mayer was convinced of it. Herschel commenced his observations on double stars in 1779, but he was not able before 1802 to decide whether the motions were rectilinear or orbital. It is curious how near Bradley was to this great discovery. He observed with the greatest care the position-angle of Castor in 1718, and again in 1759, when it had altered about thirty degrees. J. Herschel writes:—"The lapse of forty years, however, will explain this. A similar train of speculation as to the probable effect of parallax on a double star might arise afresh in his mind, and prompt the same observation on the most conspicuous and almost the only close double star known to him, with a perfect unconsciousness at that distance of time of having ever before been led by the same train of thought to the same minute scrutiny." ζ Ursæ Majoris, α Centauri (observed by Feuillée*), and γ Virginis were discovered before Castor.

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* Measures instituted for determination of the difference in parallax of two close stars must suffer from systematic errors similar to those that have been found in double-star observations. Brünnow has paid attention to this circumstance; but might not something of the same nature vitiate our parallax determinations also in other cases?

Sir W. Herschel remained almost to his death the only authority on double stars. Indeed he was without cooperators in most of the new branches of science he started. It was not till 1813 and 1819 that the subject was taken up by W. Struve in Russia, and by J. Herschel in England, who were shortly followed by a number of observers, owing to whose zealous cooperation a new problem was soon put forward—the determination of the orbits of binary stars. Sir John Herschel showed himself equal to the occasion; and we may as well consider his investigations first, although they were not so in point of time, because his method in its original form has been used more than any other up to this time, and, far from becoming obsolete, it is yearly gaining ground at the cost of those methods which have been proposed elsewhere.

The observed position-angles are laid down as ordinates, with the epochs as abscissæ, and a continuous curve is drawn through the points. This, of course, is not possible; but the points should be evenly distributed on both sides of the curve; *i. e.* in case the observations were all of equal weight, the sum of the positive deviations in the ordinates ought to be equal to the sum of the negative. Measures made by different observers are, however, of very different weight, and systematic deviations are found between them. This together with the circumstance that occasionally the tangents that follow from the distances must be taken into account, renders the construction of these curves often a task for which great experience is wanted. The beauty of the curve alone is no sufficient guide. A similar curve is constructed from the distances. These had, however, not been measured with sufficient accuracy in Herschel's time, and he computed them therefore from the first differential coefficients of the angles according to the law, that when a body is acted on by a force that emanates from a fixed point, the areas described by the radius vector are proportional to the time. The coefficients are computed from the differences of different order of the angles or from the tabulated angles themselves. Herschel, however, determined them by aid of tangents drawn to the curve at regular intervals; and this is a good method, though it takes time to do it carefully. Even when the distances have been accurately measured during the whole period embraced by the interpolating curve, they should also be deduced from the angles and the curves corrected in case of disagreement. Some investigators prefer to determine the angles from the distances by integration; but then they are liable to fall into the opposite error, and attribute undue weight to the distances. The interpolating curve for the angle is always more trustworthy. When it appears that no better agreement between the curves can be obtained, we are able to construct any number of points belonging to the apparent orbit, by reading the angles off on the curve and interpolating the most probable values of the distances between the two curves resulting from the angles and the measured distances. The area of the apparent ellipse is

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* Louis Feuillée was born in 1660, in Mane, a village in Upper-Provence, and entered the Order of the Minorites in 1680 at Avignon, in which town also his interest for astronomy was awakened. His first observations were concerning eclipses and occultations. Von Zach, from whose biography of the distinguished man these notes are taken, assures us that it was Feuillée who, in 1699, on the occasion of an occultation of θ Tauri, first remarked the phenomenon which occasionally takes place when stars are occulted by the Moon, and the star appears for seconds inside upon the Moon's disk before it finally disappears. He was also the first navigator who not only understood how to appreciate the method of determining the longitude by lunar distances, which had been proposed in the commencement of the sixteenth century, but made use of it practically as early as 1708. For this purpose he employed a so-called astronomical radius, which, Von Zach tells us, continued to be used by French sailors up to the end of the last century. Feuillée was appointed Mathematician and Botanist by Louis XIV. in 1707, and was sent out on voyages of discovery to the Levant, to Northern Africa, and South America. He thought the refraction might come out smaller there than in Europe, and made observations in Lima, from which he arrived at a value of the constant, that agrees with that long after obtained by Bouguer. He described with accuracy both the so-called "coalsacks," and gave a drawing of the largest of them. He was, in 1724, ordered out to Ferro, in order to determine the difference of longitude between this island and the Observatory of Paris, on which occasion he also ascended and made observations on the Peak of Teneriffe, which a century and a half after was made so famous by Piazzi Smyth's stay there. The remainder of his life he spent in Marseilles, where he erected a small observatory. He got a pension for life from Louis XIV., and died in 1732.

$$\tfrac{1}{2}\int_0^{\mathrm{P}} \rho^2 \frac{d\theta}{dt} dt = k\mathrm{P};$$

and as the areas described during equal times are constant, we have

$$k = \tfrac{1}{2}\rho^2 \frac{d\theta}{dt},$$

in which the right side should be multiplied by arc 1° when the angles are expressed in degrees.

From a series of values of k that follow from the Tables for the angles and distances, we find its most probable value. Knowing this, together with the angles described in regular intervals of one to five years, we may construct the isosceles triangles equal to the areas corresponding to the angles. The angles, ψ, which the tangent makes with the line perpendicular to the radius vector may be calculated as follows:—

$$\tan\psi = \frac{1}{\rho}\frac{d\rho}{d\theta} = \frac{1}{\rho}\frac{\rho^2}{2k}\frac{d\rho}{dt} = \frac{\rho}{2k}\frac{d\rho}{dt},$$

and the tangents drawn so that the triangles they form with the legs of the angle shall be equal in area to the isosceles triangles. This may serve as a guide in drawing the apparent ellipse.

The apparent orbit has hitherto been found to be as nearly elliptic as could be expected, considering the difficulty of making accurate measures; and the laws that govern the movements within the solar system hold good, therefore, in those remote spaces which the spectroscope has shown to be filled with matter in chemical composition like that of which our planet is made up. At least this is approximately the case.

Of course we do not observe the companion describing the real orbit, as we are not in the pole of it. We see it projected on the plane perpendicular to the line between the star and the solar system. The apparent orbit is then another ellipse, but the principal star does not lie in the focus. Herschel's second method starts, therefore, from the general equation of the second degree between the rectangular coordinates of points belonging to the ellipse,

$$1+ax+by+cx^2+dxy+ey^2=0.$$

If the coordinates (x, y) can be computed for at least five epochs from the observations, we can obtain the constants from these equations. Herschel gives the normal equations that result from a number of equations of the above form. He attributes equal weight to each of the equations. In fact their solution would involve rather tedious calculation if different weights were introduced, and I do not know of any investigation in which it has been tried; but I am inclined to think that this circumstance renders the second highly inferior to the first method, in which it is easy to take the weights approximately into account. I cannot, therefore, agree with those writers who consider the construction hardly worthy to be called a method. I have found it in the first instance answer better than any other in cases in which, through want of experience, I at first thought a trial unnecessary.

When the apparent ellipse has been constructed, Herschel computes the elements of the real orbit by formulæ which are very convenient. Thiele and Wilson have thought it interesting to show how this can also be done by construction, and how even the ephemeris may be drawn. When the equation of the apparent ellipse has been obtained, Herschel gives more complicated formulæ for finding the elements. These have been given in slightly different forms by Winnecke, Winagradskij, and by Seeliger.

As already pointed out, several methods had been proposed before Herschel's investigations. In Savary's* first method the elements are also obtained from five points of the apparent orbit. It is founded on the following relation between four parallel chords in the ellipse. Let the parallel chords be $2l_1$, $2l_2$, $2l_3$, and $2l_4$, where $2l_1$ is the line joining the first two given points. If the distance between l_1 and l_2 is denominated (I. II.), between l_2 and l_3 (II. III.), and so on, the relation is

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* Félix Savary was born in 1797 in Paris. He was professor at the École Polytechnique, Membre de l'Institut and of the Bureau des Longitudes. He wrote on the theory of electricity and magnetism; but while he was astronomer at the observatory, Arago suggested to him that he should give a solution of the double-star problem, and he published his two papers in the 'Conn. des Temps' for 1830 and 1832. He died in 1841, prematurely, about the same time as Flemming, in Königsberg, who was investigating the motion of Uranus with the view of discovering Neptune, and whom Bessel, in his illustrious popular lectures, saluted with a "Glück auf!"

$$\begin{aligned}&(\text{II. III.})\,(\text{II. IV.})\,(\text{III. IV.})\,l_1^2+(\text{I. II.})\,(\text{I. IV.})\,(\text{II. IV.})\,l_3^2\\ =&(\text{I. III.})\,(\text{I. IV.})\,(\text{III. IV.})\,l_2^2+(\text{I. II.})\,(\text{I. III.})\,(\text{II. III.})\,l_4^2.\end{aligned}$$

By means of this equation the angle which the conjugate diameter forms with the chords is determined, and therewith the lengths of the chords are given. Thus we know the position of four points on each side of the conjugate diameter, and the elements of the apparent orbit are obtained by elegant formulæ.

Savary also makes use of conjugate diameters in his second method, but it is nevertheless entirely different from the first. It will be remarked that in the investigations above referred to no mention has been made of the time. The epochs of the normal places have in fact only been used for determination of the epoch of periastron-passage and the period of one revolution, after all the other elements have been calculated. Now it has appeared to several astronomers very unjustifiable to make no use of so well determined a datum as the time; and it was Savary who first explained how to treat the problem in this case. Owing to the transcendent nature of the equations it can only be solved by trials. The aim to be attained is to effect these with as little labour as possible. He considered the angles and distances given for four epochs—at the time a most proper distribution of the observations, though more out of place now, after many binaries have moved through a considerable part of their orbits.

Savary starts from the equations for the three segments. That between the first and second point is, for instance, $=\frac{1}{2}AB[(E_2-E_1)-\sin(E_2-E_1)]$, where A and B are the major and minor semiaxes of the apparent ellipse and E_1 and E_2 the eccentric anomalies. It is, on the other hand, equal to the difference between the sector $k(t_2-t_1)$ and the triangle between the principal star and the two points. In the solar system k is known, and we have only six elements to determine; but in the binary systems k is not always known. Savary therefore eliminates k from the equations, expresses all the differences between eccentric anomalies in functions of E_2-E_1, and finds this quantity by trials, after which the elements of the apparent orbit are easily calculated. Savary's methods, though analytically elegant, were too complicated to be generally adopted. Encke, however, has facilitated the trials by considerations concerning the areas of the various triangles that may be calculated from the given data. His formulæ resemble more those in Gauss's 'Theoria Motus,' from which the orbits of minor planets are computed. Hence, perhaps, comes the strong predilection German calculators up to this time have evinced for Encke's method, though it has long been superseded. The trials above referred to are not the only difficulty. The problem is over-determined, eight quantities being given, seven only sought. It has to be solved for different values of one of the data in order that the four normal places may be represented. Commonly the case is such that while three complete normal places may be derived from modern observations, the fourth obtained from W. Herschel's measures gives seldom an approximation to the distance. Seeliger has therefore lately investigated the problem on the supposition that the distance is wanting for one of the epochs; but the formulæ at which he arrives are far too complicated to be generally adopted.

Thiele constructs the interpolating curves for angles and distances after Herschel's method, and finds thus the value of *k*. Then, in analogy with the calculation of planetary orbits, three complete observations suffice for the determination of the elements. To this end he makes use of Savary's equations:—

$$k(t_3-t_2)-\tfrac{1}{2}\rho_3\rho_2\sin(\theta_3-\theta_2)=\frac{kP}{2\pi}[E_3-E_2-\sin(E_3-E_2)],$$

$$k(t_1-t_3)-\tfrac{1}{2}\rho_1\rho_3\sin(\theta_1-\theta_3)=\frac{kP}{2\pi}[E_1-E_3-\sin(E_1-E_3)],$$

$$k(t_2-t_1)-\tfrac{1}{2}\rho_2\rho_1\sin(\theta_2-\theta_1)=\frac{kP}{2\pi}[E_2-E_1-\sin(E_2-E_1)].$$

The three differences between eccentric anomalies are calculated for different hypothetical values of the period and are tried by the condition that

$$(E_3-E_2)+(E_1-E_3)+(E_2-E_1)=0.$$

The important case where, besides three complete observations, one position-angle for a remote epoch is given has been treated by Thiele with equal success. The three (or rather four) equations are solved as before; but afterwards ρ_4 and E_4-E_1 are found by trial from the following equations:—

$$k(t_4-t_1)-\tfrac{1}{2}\rho_1\sin(\theta_4-\theta_1)\cdot\rho_4=\frac{kP}{2\pi}[E_4-E_1-\sin(E_4-E_1)],$$

$$k(t_4-t_2)-\tfrac{1}{2}\rho_2\sin(\theta_4-\theta_2)\cdot\rho_4=\frac{kP}{2\pi}[(E_4-E_1)-(E_2-E_1)$$
$$-\sin((E_4-E_1)-(E_2-E_1))].$$

The case is that *k* cannot be considered as definitely given when the remote position-angle has not been taken into account in determining it; but after having solved the two last equations we get a new value of *k* from the following equation, which, if it does not agree with the original value, may serve as a guide for another hypothesis:—

$$k(t_4-t_3)=\tfrac{1}{2}\rho_4\rho_3\sin(\theta_4-\theta_3)+\frac{kP}{2\pi}[E_4-E_3-\sin(E_4-E_3)].$$

The solution of the equations is facilitated by a Table originally given by Encke for E — sin E with argument E.

I propose to find epoch, period, and eccentricity from the following equations:—

$$\frac{\sin\frac{1}{2}(E''-E')\sin\frac{1}{2}(E''-E)}{\sin\frac{1}{2}(E'''-E')\sin\frac{1}{2}(E'''-E)}=A,$$

$$\frac{\sin\frac{1}{2}(E''-E')\sin\frac{1}{2}(E'-E)}{\sin\frac{1}{2}(E'''-E'')\sin\frac{1}{2}(E'''-E)}=B,$$

$$\frac{\sin\frac{1}{2}(E''-E)\sin\frac{1}{2}(E'-E)}{\sin\frac{1}{2}(E'''-E'')\sin\frac{1}{2}(E'''-E')}=C,$$

where

$$\frac{\rho\rho'\sin(\theta'-\theta)+\rho'\rho''\sin(\theta''-\theta')-\rho\rho''\sin(\theta''-\theta)}{\rho\rho'\sin(\theta'-\theta)+\rho'\rho'''\sin(\theta'''-\theta')-\rho\rho'''\sin(\theta'''-\theta)}=A,$$

$$\frac{\rho\rho'\sin(\theta'-\theta)+\rho'\rho''\sin(\theta''-\theta')-\rho\rho''\sin(\theta''-\theta)}{\rho\rho''\sin(\theta''-\theta)+\rho''\rho'''\sin\theta'''-\theta'')-\rho\rho'''\sin(\theta'''-\theta)}=B,$$

$$\frac{\rho\rho'\sin(\theta'-\theta)+\rho'\rho''\sin(\theta''-\theta')-\rho\rho''\sin(\theta''-\theta)}{\rho'\rho''\sin(\theta''-\theta')+\rho''\rho'''\sin(\theta'''-\theta'')-\rho'\rho'''\sin(\theta'''-\theta')}=C.$$

The elements which fix the situation of the real orbit are afterwards found from the angles of position in connexion with the true anomalies. This method may be used when the elements have to be deduced by the method of least squares from a great number of normal places. A table in the 'Astronomische Nachrichten' giving the eccentric anomalies corresponding to the mean anomalies for different eccentricities facilitates the trials. W. DOBERCK.

On Croll's Hypothesis of the Origin of Solar and Sidereal Heat *.

THE 'Quarterly Journal of Science' for July, 1877, contains an able and interesting article by James Croll, LL.D., F.R.S., on the age and origin of the Sun's heat. The theory of Dr. Croll may be regarded as a compromise between the mathematicians, represented by Thomson, Tait, and Newcomb, and the geologists of the uniformitarian school, represented by Playfair, Lyell, Darwin, &c. The principal points of this remarkable paper are:—

1. That, as had been estimated by Sir William Thomson and others, but twenty million years' heat could be produced by the falling together of the Sun's mass.

2. That not less than five hundred millions of years have been required for the stratification of the Earth's crust at the present rate of subaërial denudation; and hence that the gravitation theory of the origin of the Sun's heat is incompatible with geological facts.

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* From the 'American Journal of Science and Arts,' Vol. xv., April 1878. Communicated by the Author.

3. If we suppose two solid opaque bodies, each equal to half the Sun's mass, to fall together in consequence simply of their mutual attraction, the collision would instantly generate sufficient heat to reduce the entire mass to a state of vapour. If, in addition to the motion resulting from their mutual attraction, we suppose the bodies to have had an original or independent motion towards each other of 202 miles per second, the concussion would produce 50,000,000 years' heat; a motion of 678 miles per second, together with that due to their mutual attraction, would generate 200,000,000 years' heat; and a velocity of 1,700 miles per second would generate an amount of heat which would keep up the supply at the present rate for 800,000,000 years.

4. The Sun and all visible stars may have derived their heat from the collision of cold, opaque masses thus moving in space. The nebulæ are the products of the more recent impacts, and the stars have been formed by the condensation of ancient nebulæ.

5. This theory, while accepting the doctrine of the conservation of energy, indicates at the same time a possible supply of heat for several hundred millions of years, thus satisfying all moderate demands for geological time.

The mathematical correctness of the theory here stated will not be called in question. We shall consider merely the *probability* of the facts assumed as its basis. To the present writer the hypothesis seems unsatisfactory for the following reasons:—

1. The existence of such sidereal bodies as the theory assumes is purely conjectural, unless it be claimed that lost or missing stars have become non-luminous, of which we have no conclusive evidence.

2. But, granting their existence, we have no instances of stellar motion comparable with those demanded by the hypothesis—the velocity being in most cases less than 50 miles, and in no case exceeding 200 miles per second.

3. If the two masses by whose collision the Sun is supposed to have been formed were very unequal, as would be most probable, the amount of heat generated would be correspondingly less.

4. Such collisions as the theory assumes are wholly hypothetical. It is infinitely improbable that two cosmical bodies should move in the same straight line; and of two moving in different lines, it is improbable that either should impinge against the other. Comets pass *round* the Sun without collision against it.

5. But granting these difficulties removed, let us suppose that about 800,000,000 years ago, two cold, opaque bodies, each containing one-half the matter of the solar system, were approaching one another in the same straight line, each at the rate of 1,700 miles per second*; that on meeting, their motion was transformed into heat; and that their united mass was at once reduced to vapour: the great question yet remains—How much of the period repre-

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* This is the greatest velocity mentioned by Dr. Croll. An increased rate of motion would, of course, produce more heat; but the hypothesis would be open to the same objections.

sented by these 800,000,000 years' heat can be claimed as *geological* time? The nebula formed by the collision would extend far beyond the present orbit of Neptune. The amount of heat radiated in a given time from so vast a surface would doubtless be much greater than that now emitted in an equal period. No considerable contraction could occur until a large proportion of the heat produced by the impact had been dissipated in space. It has been shown by Trowbridge* that with a temperature at the Sun's surface of twice its present intensity the solar atmosphere would be expanded beyond the Earth's orbit. The conclusion seems inevitable that much the greater part of the 800,000,000 years' heat must have been radiated into space before the planets were separated from the solar mass, and consequently that the amount of geological time cannot, to any great extent, have exceeded the limits indicated by the researches of Sir William Thomson.

Upon the whole, it seems more difficult to grant the demands of Dr. Croll's hypothesis than to believe that in former ages the stratification of the Earth's crust proceeded more rapidly than at present. The former, as we have seen, has no sufficient basis in the facts of observation. On the other hand, if our planet has cooled down from a state of igneous fluidity, the great heat of former times must doubtless have intensified both aqueous and atmospheric agencies in producing modifications of the Earth's exterior.

Bloomington, Indiana,
March 1878.

DANIEL KIRKWOOD.

On the Aerolitic Epoch of November 12th–13th†.

It is now well known that clusters of small meteors—the so-called shooting-stars—move in elliptic orbits about the Sun. Catalogues of fire-balls and meteoric stones indicate, moreover, that groups of *larger bodies*, somewhat widely dispersed, revolve in like manner about the centre of our system, their orbits in certain cases intersecting that of the Earth. The 12th and 13th of November is one of these *aerolitic epochs*, the date being nearly coincident with that of the great November shower of falling stars. The writer until recently supposed the meteorites of this epoch to revolve in the same orbit with the nebulous swarm which furnished the showers of 1833, 1866, and 1867‡. Later study of the facts, however, has rendered the theory of this intimate relation extremely improbable. The principal phenomena of this epoch (not including star-showers) are the following:—

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* Proc. Amer. Phil. Soc., Vol. xvi. pp. 329–333, and National Quarterly Review, March 1877, p. 292.

† Paper read before the American Phil. Soc. Communicated by the Author.

‡ A list of stone-falls, detonating meteors, and large fire-balls which have appeared about this epoch is given in 'Meteoric Astronomy,' pp. 58–60.

(*a.*) 1582, meteoric phenomena at Zurich.

(*b.*) 1765, an extraordinary meteor at Frankfort.

(*c.*) 1820, a detonating meteor seen in Russia.

(*d.*) 1822, fall of aerolites at Potsdam and Leipzig.

(*e.*) 1828, a great meteor seen in full sunshine in France.

(*f.*) 1835, a fall of aerolites in France.

(*g.*) 1849, a fall of aerolites at Tripoli.

(*h.*) 1849, a large meteor seen in Mecklenberg.

(*i.*) 1856, a meteoric stone fell in Italy.

(*j.*) 1877, a brilliant meteor seen in Arkansas and another in Wisconsin.

REMARKS.

(*a.*) This so-called "fall of fire from heaven" occurred on the 28th of October, O. S., or November 7th, N. S. Making allowance for the precession of the equinoxes, the date corresponds at present to the morning of November 12th.

(*b.*) This bolide was observed November 11th, and is the only one in our list which occurred *very* near the epoch of the great star-shower in November.

(*c.*) See Greg's catalogue of fire-balls and meteoric stones; also Quetelet's catalogue of shooting-stars.

(*d.*) Several aerolites fell at this date near Potsdam, and also at Taucha, near Leipzig, about 75 miles distant.

(*e.*) See Quetelet's catalogue.

(*f.*) This fall of aerolites occurred on the evening of November 13th, in the Département de l'Ain, France. The meteor was *unconformable* to the radiant of the Leonids, its motion being from south-west to north-east. A fragment is in the collection of Prof. Shepard, of Amherst, Massachusetts.

(*g.*) The meteoric phenomena of this date are thus described in the catalogne of Mr. Greg: "Seen in the southern sky. Varied in colour; a bright cloud visible one and a half hour after; according to some a detonation heard fifteen minutes after bursting. Seen also like a stream of fire between Tunis and Tripoli, where a shower of stones fell; some of them in the town of Tripoli itself."

(*h.*) This fire-ball appeared on the same evening or night.—Greg's catalogue.

(*i.*) This aerolite fell at Trezano. A fragment is in the collection of Professor Shepard.

(*j.*) A large meteor was seen by Professor Robert C. Hindley, of Racine, Wisconsin, on Sunday evening, November 11th, at three minutes past six o'clock (Chicago time?). This meteor is thus described by Professor H. in the 'Scientific American' for December 1, 1877: "Direction N.N.E.; altitude at commencement of course about 30°, length of course from 10° to 12°; time of falling about 8 seconds. It fell towards the west, making an angle in falling to the earth of about 65° with the vertical passing through the body. During the latter three-fourths of its course, its length, including the luminous trail, was about one half of a degree. The nucleus was very brilliant; its colour at first a yellowish white, then a light green, and lastly, a greenish-yellow. Could its colour have been due to boron, thallium, &c.? I find no record in any of the numerous analyses of meteoric stones of the presence of elements likely to give the green colour."

On the following evening, November 12th, at 6h. 36m. (Memphis time), Frank L. James, Ph.D., M.D., of Osceola, Arkansas, saw another meteor in the same part of the heavens, and in some respects so strikingly resembling that observed in Wisconsin, that he was disposed, on reading Prof. Hindley's description, to think they had observed the same phenomenon, and that one or the other had mistaken the date. I have, however, corresponded with both the gentlemen, and have found that the meteors were seen on different evenings. "The date is fixed," says Dr. James, "not only by my own 'case-record' but by that of a friend and brother physician who assisted me in an amputation on the previous day." The following account of the Arkansas meteor is extracted from Dr. J.'s communication in the 'Scientific American' for December 29th, 1877:—"I was startled by a sudden glare of light which seemed to come from right in front of me. Throwing up my eyes I saw a large and very brilliant meteor in the north east, falling apparently nearly straight downward, with a slight deviation to the east. When I first saw the meteor it was about 30° in height; and judging from the length of time it took to travel the remainder of its course, it must already have fallen 3° or 4°. It fell through an arc of about 12° or 15° in all, and was about ten seconds in falling. When I first saw it it had a golden hue, which suddenly changed to green, of that peculiar shade produced by burning chlorate of potash with nitrate of barium and sulphur. The light shed by it was pulsating and sufficiently powerful to light up the Tennessee shore and the sand bars, so as to show every log and stump."

PROBABLE INFERENCES.

1. The number of stone-falls and detonating meteors observed on the 11th, 12th, and 13th of Novmeber is more than double the average daily fall. Hence the periodic return of a cluster whose orbit intersects that of the earth is rendered highly probable.

2. None of the aerolites or meteors of the preceding list are known to have been conformable to the radiant in Leo, while those of November 13, 1835, and November 12th, 1877, were certainly *un*-conformable, their heliocentric motion having been direct. This aerolitic group cannot therefore be connected with the shooting-stars of November 14th.

3. These facts, it must be confessed, are unfavourable to the hypothesis, formerly advocated by the writer, that 'meteoric stones are but the largest masses in the nebulous rings from which showers of shooting-stars are derived"*. It is true that in the great star-

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* Meteoric Astronomy, p. 64.

showers of 1799, 1833, and 1866 a number of large fire-balls were seen which belonged undoubtedly to the cluster of Leonids; but it is remarkable that among all this number no detonation was ever heard, and that no meteoric stones have ever fallen during these extraordinary star-showers.

4. The dates of the phenomena given above indicate a period of seven years. Several sporadic fire-balls, however, have appeared at this epoch, and no definite conclusion in regard to the period is possible without additional data.

DANIEL KIRKWOOD.

The Variable Star R Scuti.

THE observations on this interesting variable extend from July 7 to December 3, 1877, when further observations were rendered impossible, owing to the star's near approach to the sunset horizon. The value of the series is somewhat impaired by serious breaks at critical points of the light-curves between July 24, August 3, August 11–28, and October 13–27.

The results when charted, however, exhibit two maxima and two minima. The first maximum, occurring about September 19th, is deduced from a very good series of observations, which show a very gradual but slow increase in light; while the second maximum, occurring on or about November 17th, is represented by a very rapid but uniform increase, followed by a slight decrease and a complete standstill until the observations terminated. In the case of the second maximum the light apparently reached its greatest brilliancy on November 7th, according to the observed brightness, but the general characteristic of the light-curve indicated the maximum as occurring on the 17th. Errors of observation may possibly account for the slight decrease noticed after the 7th, owing to the moonlight which prevailed after the 9th.

The first minimum is only fairly indicated as occurring about August 17th, the second October 18th, but neither is to be taken as showing any decided results but only approximations. The comparison stars used were taken from Argelander's list ('Bonner Beobachtungen,' Vol. vii.) and their light-values were independently determined from my observations, and the corresponding brightness of R Scuti deduced from these. For comparison my table of light values for these stars is given, together with those of Argelander and Seth C. Chandler, Jr.

Comparison Star.	Argelander. Light-value.	Chandler.	Sawyer.
k........	23·1	—	24·0
h........	19·8	20·0	20·0
g........	18·0	19·0	17·6
c........	16·0	15·0	14·1
m........	12·9	12·5	12·6

The following are the light-values for each evening:—

1877.		1877.		1877.	
July 7	13·3	Sept. 1	18·0	Oct. 13	15·8
8	15·9	2	18·5	27	16·3
9	16·8	4	20·3	28	18·8
10	16·4	6	19·3	30	19·5
11	18·5	9	20·3	Nov. 3	22·0
12	17·6	10	20·3	4	22·0
14	17·9	11	20·3	6	22·5
15	16·9	12	21·5	7	23·2
23	18·8	14	21·5	9	23·0
24	19·3	21	22·0	13	22·5
Aug. 3	15·3	22	22·0	14	22·0
4	16·3	23	21·5	18	22·0
6	15·8	24	22·0	19	22·0
8	15·5	30	17·8	20	22·0
10	15·5	Oct. 5	18·8	30	22·0
11	16·3	6	18·8	Dec. 1	22·0
28	17·3	7	16·3	2	22·0
30	18·0	10	15·3	3	20·7

A low-power opera-glass was used throughout the series. Observations made at Cambridgeport, Mass. EDWIN F. SAWYER.

Cambridgeport, June 1878.

CORRESPONDENCE.

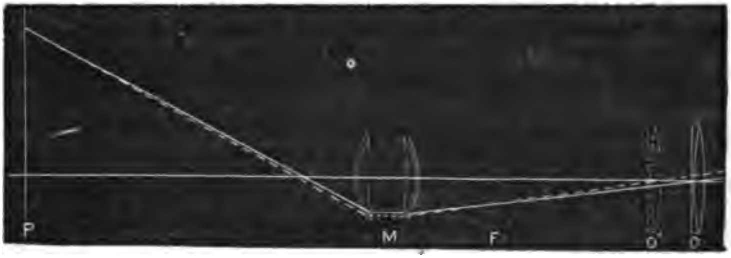
To the Editor of 'The Observatory.'

Distortion of the Photoheliograph.

SIR,—

The inquiry which was made by Mr. Common at the Astronomical Society's Meeting, as to the possible discordance in the corrections for distortion (1) of a photographic image of the Sun, (2) of image of De La Rue's Scale, was quite pertinent; but it was evident that it could not very well be answered there.

I now offer a diagram relating to it and give the following explanations:—



The firm lines relate to the Scale, the chain-lines to the Sun; O, O′ are the two positions of the object-glass for the Scale and Sun respectively. F is the principal focus or place of first image, M the magnifying doublet, P the photographic plate on which the magnified image falls.

The focal adjustment is made by sliding the object-glass tube. This must be slid so far that the first image falls at a definite position F, the rays from which, passing through the magnifying doublet, will make a clear image on the photographic plate. For the Scale the central ray is easily traced. For the Sun, draw the straight chain-line from the place of first image F, through the centre of chain-line object-glass O′, and continue it to the Sun. This line is slightly inclined to the ray from the Scale. But, considering its path towards the photographic plate, all rays which cross at F, the place of first image, do unite on the photographic plate P.

And whatever be their origin, they are subject to the same distortion there: it is given only by the magnifying doublet. But the distortion for a given direction towards the Sun is the same as for a direction slightly different towards the Scale.

Therefore if we used a formula for distortion depending on the instrumental *angle*, it ought to be different for the Sun and the Scale.

If we used a formula depending on the *measure of photograph*, it ought to be the same. This is our practice.

I am, Sir, yours faithfully,
G. B. Airy.

Royal Observatory, Greenwich,
1878, July 13.

Transit of Mercury.

Sir,—

The last phases of this phenomenon were observed under rather favourable circumstances at the Melbourne Observatory. At sunrise the planet was well on to the Sun's disk. With the 8-inch refractor a careful watch was kept for possible satellites, and for appearances at internal and external contact at egress. No signs of a satellite were observed; the internal contact took place at $20^h\ 24^m\ 52^s$, Melbourne mean time and external at $20^h\ 28^m\ 0^s{\cdot}6$; 57 seconds before internal contact a *fine thread*, like what I observed in the Transit of Venus, was seen to join the planet and Sun's edge, the planet remaining quite round and the Sun's limb unbroken. No part of the planet could be seen outside the Sun's limb, nor any luminosity around its border, as in the case of Venus. I suspected a central white spot on Mercury, but fancy it was an optical illusion.

From measures of α Centauri taken on May 8, it appears that the components are slowly separating.

I am, Sir, yours faithfully,
Robert L. J. Ellery.

Melbourne Observatory,
1878, May 13.

Early Sun-spot Drawings.

Sir,—

Between the introduction of the telescope, in the year 1610, and the first recorded observation of a Transit of Mercury, there happened three passages of the planet across the Sun's disk. The dates of these were:—1615, May 3; 1618, November 4; 1628, May 5. All were, more or less, visible in Europe. Remembering the ease with which I have perceived Mercury when in transit with a very small telescope, it has struck me that there may be drawings of the Sun's disk extant for the dates just mentioned, so that the small black dot of Mercury may be marked on them without the observer being aware that he was recording an observation of the planet's transit. The only early solar-spot diagrams I have been able to see are those by our countryman Harriot, for about the year 1611, those in Galileo's 'Macchie Solari' for 1613, and those in Scheiner's 'Rosa Ursina' for the year 1625. There are, no doubt, however, other works giving the like. Humboldt, in 'Cosmos,' mentions two such of this period, one by Jean Tarde and the other by a Belgian Jesuit. Have any of your readers access to any sun-spot drawings, or any works containing the like for the years 1615, 1618, 1628?

Faithfully yours,
S. J. Johnson.

Upton Helions Rectory,
Crediton, July 10.

NOTES.

Dudley Observatory, Albany.—Mr. Lewis Boss was appointed Director of this Observatory, which is affiliated to the Union University, in July 1876, and now issues a report on the state of the instruments and work done in the year 1877. The instrumental equipment consists (1) of a 13-inch equatoreal by Fitz, of New York, which, on account of optical defects and faulty construction, has never been considered as well adapted for astronomical research; (2) of the Olcott Meridian Circle, which has suffered from lack of proper care, but is now in fair working order, though the performance of the object-glass is far from satisfactory; and (3) of the Transit instrument, which is capable of furnishing excellent results in the determination of Right Ascensions. There are also a comet-seeker of four inches aperture, various clocks, two chronographs, both entirely out of repair, and self-recording meteorological instruments not in use. The staff consists of the astronomer, Mr. Lewis Boss, a student assistant, Mr. O. H. Landreth, and a janitor; and, in addition to his other duties, the astronomer has to deliver a course of lectures in the Union University, which occupies a considerable portion of his time, during April, May, and June. "With such a force and under the circumstances," remarks Mr. Boss, "I felt that it would be unwise to commit the Observatory to any extensive series of astronomical observations which would require years for their completion. The work has therefore been more or less of a fragmentary nature, but not, I hope, without scientific value." With the Olcott circle, observations of Mars for parallax, according to Prof. Eastman's programme, were made on 36 nights, a number of selected stars were observed, including those used by Mr. Gill in his observations of Mars and Melpomene at Ascension, and in addition some observations of Ariadne, Melpomene, and Iris were made. With the equatoreal, Mr. Landreth has made 22 sketches of Mars and a large number of measures of position-angles of his polar spot, and also seven complete sets of differential observations of Iris east and west of the meridian, for determination of the Solar parallax, during the very favourable opposition of Nov.–Dec. last, when this minor planet was distant from us only seven-eighths of the Earth's mean distance from the Sun. The instrumental difficulties experienced were, however, very great, and the personal fatigue incurred proved no insignificant obstacle to success. A few phenomena of Jupiter's satellites were also observed. The computations generally are to be pushed forward and the results prepared for publication as fast as they become available. The Dudley Observatory sends time-signals to the New York Central Railway and to the towns on that line, without, however, receiving any compensation for the expense incurred, a state of things which the Director thinks ought to be remedied. Another matter needing reform was the admission of visitors, the pressure of applicants having become so great that in a single evening 116 persons had gained access to the Observatory, whilst their conduct was sometimes such as to render their presence any thing but agreeable. It was therefore found necessary to place some restrictions on the issue of cards. For the coming year Mr. Boss intimates his readiness to undertake with the Olcott circle the observations of a zone of 6° still vacant in the programme of the *Astronomische Gesellschaft*, provided a temporary increase can be made in the staff, which would necessitate an additional expenditure of 2000 dols. per year for three years, a sum which the Director trusts may be raised by public subscription. The equatoreal may with advantage be directed to observations of phenomena of Jupiter's satellites, and, if a good position micrometer be provided, to a series of measures of their positions and distances.

In his report Mr. Boss attaches much importance to the formation of a good astronomical library, which he regards as quite as indispensable as good instruments, in order that the astronomer may profit by the experience of his predecessors and contemporaries. The library had been allowed to fall into a most deplorable state; but it has now been put in order, and the Director hopes for considerable additions by donations, which are usually distributed with a liberal hand among the working observatories of the world.

UNDULATIONS IN THE TAIL OF COGGIA'S COMET, 1874*.—Mr. Trouvelot, on July 21, 1874, at $9^h\ 0^m$, observed a bright ray of light darting from the north-west horizon, which he at first took for an auroral streamer, but afterwards found, on the pheuomenon being repeated at frequent intervals, that it arose from a remarkable pulsation in the tail of Coggia's comet, the head of which was then below the horizon. For more than an hour the tail kindled and extinguished more than a hundred times, being sometimes so bright that in spite of the moon (then in her first quarter) it could be distinguished in all its contours to its very extremity, and at other times so faint that not a trace could be seen. Mr. Trouvelot adds that, whether it be coincidence or not, a strong magnetic perturbation was going on in Cambridge, U.S., though no auroral light was seen. Such pulsations have been previously remarked in the tails of other comets; but there is some doubt as to their being any thing but subjective phenomena, having their origin in the varying sensitiveness of the observer's eye.

THE TRANSIT OF MERCURY, 1878, MAY 6†.—M. André's report of the observations made by the French Expedition to Utah has been presented to the French Academy. M. André and M. Hatt had each a 6-inch refractor with which experiments were to be tried on the black drop or dark ligament by varying the aperture and the depth of the dark glass used. The expedition was also provided with a photoheliograph under the charge of M. Angot, and a Washington long-focus heliograph, with the object of comparing the photographic methods employed at the late Transit of Venus by the Americans and by other nations. Unfortunately a violent storm prevented observations of ingress, and it was not till noon that any photographs could be taken. By taking advantage of every break in the clouds M. Angot succeeded in obtaining 48 photographs, and Mr. Hoffman, whose instrument had suffered injury during the storm, secured 30.

The Egress was well observed by M. André and M. Hatt. The latter with the full aperture of 6 inches saw the first trace of a ligament at $3^h\ 14^m\ 9^s$, Ogden M.T., but by using a darker part of his graduated wedge he made this ligament disappear; soon after it reappeared and he made it again disappear in the same way, and so on up to geometric contact, which he observed at $3^h\ 14^m\ 28^s$. M. André at first limited his aperture, by means of a diaphragm, to 4 inches, and noted, at $3^h\ 14^m\ 5^s$, the first trace of a ligament, which after a few seconds became very broad and dark. On removing the diaphragm the ligament was reduced by more than half, and on replacing the diaphragm and using the graduated dark glass, the ligament was made to disappear and again reappear. Contact was observed at $3^h\ 14^m\ 32^s$, 4^s after M. Hatt. External contact was observed by M. André at $3^h\ 17^m\ 18^s$, and by M. Hatt at $3^h\ 17^m\ 25^s$. M. André concludes that by following the simple method used by M. Hatt contact in a Transit of Venus may be certainly observed within 2^s, being much easier to observe than the corresponding phenomenon for Mercury. At Palermo the contacts at ingress were observed spectroscopically by Prof. Tacchini, and telescopically by MM. Cacciatore, Delisa, and Agnello. For external contact the spectroscope gave $4^h\ 4^m\ 35^s{\cdot}5$, and the telescopes $4^h\ 5^m\ 39^s{\cdot}9$, difference $1^m\ 4^s{\cdot}4$; and for internal contact $4^h\ 7^m\ 46^s{\cdot}7$, and $4^h\ 7^m\ 36^s{\cdot}9$ respectively, difference $9^s{\cdot}8$ in the opposite direction. Prof. Tacchini first saw the planet on the chromosphere at $4^h\ 0^m\ 33^s$. The difference $9^s{\cdot}8$ would imply that the Sun's radius seen in the spectroscope is smaller by $0''{\cdot}68$ than the telescopic radius, a quantity which agrees with that found from a comparison of transits observed with the spectroscope and transit-circle respectively, from April 12 to May 9, namely $0''{\cdot}42$. The Transit of Venus in 1874 gave $4''{\cdot}33$ and transits of the Sun about the same time $2''{\cdot}82$, whilst a year later, in 1875, transits observed spectroscopically and with the transit-circle gave $1''{\cdot}86$, from which Prof. Tacchini concludes that the Sun's diameter varies with the sun-spot period.

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* Amer. Journ., February 1878.

† Comptes Rendus, Vol. lxxxvi. Nos. 20, 22.

Observations of the Transit have also been made by a large number of observers in the United States, including Prof. H. Draper and Mr. Rutherfurd, who secured a number of photographs, Prof. Young, Prof. Peters, Prof. Pickering, and at the Washington Observatory by Professors Newcomb, Hall, and Eastman. From Prof. Newcomb's observation of internal contact at ingress, the predicted time by Le Verrier's tables was only 10^s late. Prof. Peters believes he has obtained indications of an atmosphere round Mercury. Prof. H. Draper, on the other hand, saw neither atmosphere nor bright spots, nor did he see the planet before first contact on the solar corona, as Prof. Langley, M. Janssen, and others did.

At Toulouse M. Perrotin, observing through dense cloud without a dark glass, saw a penumbra or dark aureole round the planet on three different occasions, but this disappeared when the light became too bright.

THE ULTRA-VIOLET IN THE SOLAR SPECTRUM [*].—M. A. Cornu has been engaged on a careful study of the ultra-violet end of the solar spectrum, which has led him to some important conclusions. In this work he has prepared two plates in extension of Ångström's maps, and on the same scale, the first from *h'* (4101·0) to O(3439·7), observable with an ordinary spectroscope having prisms and objectives of glass; the second from O to U (2948·0), observed photographically by means of a spectroscope with objectives of quartz and a prism of Iceland spar, all carefully worked. He has thus mapped out the spectrum to a considerable distance beyond the limit of previous investigators, M. Mascart having only been able to observe as far as T (3019·7), and that with great difficulty. But the most important result of M. Cornu's work is that there is a limit to the solar spectrum, depending not on the absorption of the prism and objectives, but of our atmosphere; and the amount of this absorption may be judged of by the circumstance that whilst with an Iceland-spar prism of 60°, rays in the spectra of metals may be observed at a deviation of more than 70°, the solar spectrum cannot be photographed beyond 60° of deviation. This absorption by our atmosphere is, of course, much greater when the Sun is low, and it is only about noon at midsummer that the spectrum can be traced as far as U; in winter the photographs barely extend as far as T. But M. Cornu finds that at equal altitudes the absorption is much greater in summer than in winter—a fact which he explains by the presence of a much larger amount of vapour at the former season, the absorption being due to aqueous vapour. In fact there is nearly seven times as much vapour in the air in summer as in winter.

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[*] Comptes Rendus, Vol. lxxxvi. No. 2.

MAGNETIC AND ELECTRIC ACTION IN THE SUN'S ATMOSPHERE*.—From a discussion of his spectroscopic observations, M. A. Cornu has been led to the conclusion that the position and relative brightness of the dark lines in the solar spectrum may be explained by the action of an absorbing atmosphere similar in its constitution to volatilized meteorites. From this he deduces the following consequences:—1. It is probable that the enormous mass of the Sun's atmosphere, containing, like the meteorites, a large proportion of vapour of iron, exercises an appreciable action on terrestrial magnetic phenomena, so that the diurnal variations of the needle would, on this view, be due to the direct magnetic action of the Sun. Although iron when heated to incandescence loses its attraction on the magnet, M. Cornu urges that it has not been proved that the attractive force is reduced rigorously to zero, and there may be sufficient left to exercise a very appreciable action on the Earth. 2. The Earth, having probably a common origin with the Sun, may also largely be composed of iron; and M. Cornu concludes that the central portion of the Earth being, as is known, much more dense than the exterior, and therefore presumably metallic, consists in large proportion of iron—a supposition which would give an explanation of terrestrial magnetism. M. Cornu further attributes similar constitution to the Moon on the strength of a feeble but unequivocal action on the magnetic needle. 3. Considering that the solar prominences show a spectrum identical, in the relative brightness and sharpness of the lines, with that of highly rarefied hydrogen rendered incandescent by the electric discharge, it appears probable that, appearing as they do in the neighbourhood of faculæ and spots, they simply represent massses of gas traversed by induction currents in the neighbourhood of magnetic or electric regions in rapid motion, and that, as M. Fizeau has already pointed out, they are analogous to the terrestrial aurora. The prominences being, on this view, simply the illumination by induction currents of masses of rarefied gas, their rapid extension and sudden disappearance would be explained without recourse to the improbable hypothesis of jets of gas having velocities of hundreds of miles a second.

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* Comptes Rendus, Vol. lxxxvi. No. 8.

SOLAR PROMINENCES AND SUN-SPOTS*.—The spectroscopic observations of the *b* and 1474 *k* lines on the Sun's limb made by Prof. Tacchini in 1877 compared with those of previous years, commencing with 1872, have led him to the following conclusions:—

1. The mean daily number of positions of *b* and 1474 *k* shows a minimum in accordance with that of the sun-spots, having fallen for *b* from 40·0 in 1872 and 42·3 in 1873 to 25·3 in 1877, and for 1474 *k* from 42·2 in 1872 and 44·7 in 1873 to 35·8 in 1877.

2. From maximum to minimum the diminution in the visibility of *b* is greater than in that of 1474 *k*, so that *k*–*b* increases steadily from 2·2 in 1873 to 10·5 in 1877.

3. The vapour of iron has an enormous preponderance over those of other substances at the base of the chromosphere; and after this comes magnesium, though its atomic weight is much less. Other substances have almost disappeared at the epoch of minimum. The preponderance of iron, with alternations of maxima and minima, may, according to Prof. Tacchini, serve to explain the parallelism between the sun-spot curves and those of terrestrial magnetic variations.

Prof. Tacchini's observations of sun-spots in the first quarter of 1878 show that the frequency-number has fallen off regularly from 6·75 in the first quarter of 1877 to 2·50 in the corresponding period in 1878, whilst the proportion of days without spots has increased from 0·34 to 0·60. Thus we would seem not yet to have reached the minimum. In the period from 1878, Feb. 10 to March 2, neither spot nor pore was observed, but the granulation was splendid. With regard to the prominences, the number is also very small, being on the average 2·1 a day, with a mean height of 30″. The distribution in heliographic latitude shows two maxima in the zones 30° to 60° N. and S., the numbers for these being 23 and 22; whilst for the equatoreal zones 0° to 30° they are 5 and 8, and for the polar zones 6 and 3. Most of the prominences were faint, with a nebulous structure, and no metallic eruptions have been observed, with the single exception of a limited metallic spectrum, on 1878, March 2, near some bright chromospheric jets announcing the arrival of a group of faculæ and a small spot, seen the next day.

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[*] Comptes Rendus, Vol. lxxxvi. Nos. 12, 16.

Observations of Mercury in 1798*.—In his Tables of Mercury, Le Verrier has only used meridian observations made since 1800, and it is therefore of interest to see how earlier observations are represented. Towards the end of last century Vidal, at Mirêfoix, observed Mercury with great assiduity, and though he only had a telescope of 2½ inches aperture, he succeeded in the year 1798 in obtaining 102 meridian observations in 230 days, the planet on one occasion being within a diameter and a half of the Sun. These observations, to the number of 100 of the Sun and as many of Mercury, M. Bigourdan has reduced and compared with Le Verrier's tables. The mean resulting correction in R.A. to the Solar Tables is $+0^{s}{\cdot}06$, and to those of Mercury $+0^{s}{\cdot}12$, the extremes being $+0^{s}{\cdot}42$ and $-0^{s}{\cdot}11$.

The comparison shows that the tables represent the observations fairly, and that Vidal observed with as much precision as could be expected with the means at his disposal.

Cincinnati Observations.—Under the Directorship of Mr. Ormond Stone, 517 double stars have been measured with the 11-inch refractor of this Observatory during the year 1877, and the results are now published with praiseworthy promptitude. In the Introduction is given a careful discussion of Personal Equation both in position-angle and distance. The result seems to indicate that the error in the former case depends almost entirely upon the position of the observer's eyes with reference to the line joining the star. Three observers took part in the measures, and the observations as printed have been referred to the mean of the three by correcting for personal error in both elements. A comparison with previous results obtained by various well-known double-star observers shows that the distances as measured at Cincinnati are presumably free from systematic error, agreeing with the mean values for stars which have no sensible orbital motion. In conclusion, Mr. Ormond Stone gives a list of stars which show a difference of more than 3° in position-angle between the Cincinnati measures and former results, with a view to their further observation as suspected binaries.

A Minor Planet was discovered on June 26, by Dr. Peters, of Clinton, New York. This is the 188th member of the group, and the 29th planet discovered by that astronomer.

New Comet.—Mr. Lewis Swift, of Rochester, U.S., discovered at 2 A.M. on July 7, a large faint comet in R.A. $17^{h}\ 40^{m}$, N.P.D. 72°, with a slow motion south-west. It had no tail nor nucleus, but a central condensation.

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* Comptes Rendus, Vol. lxxxvi. No. 16.

Tempel's Periodical Comet.—We learn from a telegram received from Admiral Mouchez, of the Paris Observatory, that this comet has been detected by M. Tempel at the Arcetri Observatory, near Florence. It was first seen on July 19 at $9^h\ 30^m$ P.M., its observed approximate position at that time being—R.A. $15^h\ 17^m$, N.P.D. 94° 15′. No mention was made of its intrinsic brightness.

Capt. Mouchez has been appointed Director of the Paris Observatory and promoted to the rank of Admiral, with M. Loewy as Sub-director. M. Janssen has succeeded M. Puiseux as member of the astronomical section of the Bureau des Longitudes, vacating the post of geographer which he previously held.

Mr. Thornthwaite's 'Hints on Reflecting and Refracting Telescopes,' of which the third edition has been recently published, contains much useful information on the adjustment of telescopes, a matter in which amateurs who observe with reflectors will be thankful for some assistance. The small mirror in the Newtonian or Cassegrainian is peculiarly liable to derangement, but with the help of the hints here given little difficulty will be experienced in setting matters right. There are also directions for silvering specula and for roughly adjusting an equatoreal, descriptions of various forms of cheap mountings, and a useful list of test-objects. A simple form of Observatory, known as the Romsey, is also fully described with illustrations, which will enable any amateur to set one up for himself.

Editor.

Meteor Notes for August.

Observations may be made in the mornings of the first ten days and resumed in the evenings after the 20th. The moon does not set till $1^h\ 14^m$ A.M. on the 10th, and $2^h\ 29^m$ A.M. on the 11th, so that we can hardly expect a good view of the *Perseids* this year. The radiant-point was suspected to change its position in 1877 from 40° +56° on Aug. 3–7, to 50° +55° on Aug. 12, and to 60° +59° on Aug. 16. The duration of the shower is uncertain. A few meteors fall from the same centre near η Persei late in August and early in September, but they may form a distinct shower or be a deflection from the original stream.

In August we have numerous showers occurring from the east and north-eastern constellations, many of them being situated in Auriga, Camelopardus, and Perseus. Some of these have been very scantily observed hitherto, for they only come well into play after midnight, and observers seem to have had their attention mainly diverted to the *Perseids*. We now require observations of the minor systems of the August meteor-epoch; and as a preliminary guide to some of the most important of these the following may be mentioned as having been deduced by the writer from a mass of foreign observations. The showers are all eastward of the *Perseids*, and the dates are from Aug. 6 to Aug. 12; the figures in brackets give the number of meteors proving each radiant:—96° +71° (87), 70° +64° (74), 61° +39° (59), 61° +48° (59), 51° +74° (62), 78° +56° (59), 76° +45° (43), 50° +47° (42), 92° +57° (42), 134° +77° (30), 74° +33° (28), 99° +45° (17), 45° +33° (18), 52° +20° (14). Further west there are a vast number of contemporary showers, prominent among which are the *Lacertids* with a centre at 334° +48° (and closely bordering shower at 330° +42°), and the *Andromedes* I. at 7° +33° (Greg). Last year a strong shower of swift meteors with streaks was traced chiefly on Aug. 12 from 31° +18°.

Recent Observations.—Mr. E. F. Sawyer, of Boston, Mass., observed three stationary meteors in the early part of this year, namely (I.) on Jan 25, at 162° +3°, (II.) on Jan. 28 at 119° −6°, and (III.) on Feb. 24 at 145° +8°. I. agrees with a shower seen by Tupman on Jan. 8–10, 1870, at 165° +4°; II. with one observed by the writer in Feb. 1877 at 120° −5°; and III. with a radiant deduced from the Italian observations (1872) at 147° +4° for Feb. 1–Mar. 12. III. also nearly coincides in position with a stationary meteor observed at 146° +9° on April 3, 1872, by Signor Zona, at Caltanisetta, Italy, and with a rich shower traced by the writer on the morning of Dec. 8, 1877, at 145° +7°.

In our July "Notes" two showers (36° +47° and 12° +70°) were mentioned as requiring further observation; these are now well verified. A full examination of the meteor-paths recorded by foreign observers in the mornings of July 25–31 has revealed, amongst others, two extremely certain radiant-points, each with 35 meteors, at 33° +51° and 15° +70°.

Mr. V. Cornish, of Debenham, Suffolk, writes that in daylight on June 29 $9^h\ 7^m$, he saw a fine meteor "descending where the sun had set not long before." On the same evening at 10^h another was seen, brighter than Arcturus. Its course, as far as could be made out, was from midway between Cepheus and Cassiopeia to 5 Lacertæ. Path about 15°, no train. On June 30 and July 1 several fine meteors were seen and described to him by a friend:—

June 30, $10^h\ 25^m$, = Altair, midway between Vega and α Coronæ.

June 30, $10^h\ 30^m$, = Vega, among clouds at ε Cephei.

July 1, $10^h\ 1^m$, = Vega. A bright star was seen slightly to the left of Altair. It simply shone out and then disappeared without perceptible motion. [A stationary *Aquilid.*]

July 1, $10^h\ 19^m$, = α Cephei. Path curved from β to α Cephei.

On June 24, $12^h\ 30^m$, = Jupiter, descending close by η and α Persei. Alt. about 15°. Very short path.

W. F. Denning.

Aug.	G. M. T.	PHENOMENA.
	h m	
15. Th.	11 42	♃ 4 Sat. Oc. D.
	14 17	8276 Oc. R. 272°
16. F.	8 58	♃ 3 Sat. Tr. I.
	11 12	♃ 3 Sat. Sh. I.
	12 34	♃ 3 Sat. Tr. E.
	13 4	♃ 1 Sat. Oc. D.
	15	☾ Apogee.
17. S.	10 12	♃ 1 Sat. Tr. I.
	10 47	♃ 1 Sat. Sh. I.
	12 32	♃ 1 Sat. Tr. E.
	13 7	♃ 1 Sat. Sh. E.
	8 29	♄ rises.
	10 22·2	♃ tr. D. 20° 58′ S.
	14 15	♀ rises.
	14 14·7	♈ transits.
	14 25·8	♄ tr. D. 1 30 S.
	16 52	☉ rises.
	22 12·5	♀ tr. D. 20 41 N.
18. S.	0 3·7	☉ tr. D. 13 5 N.
	1 40·2	☿ tr. D. 0 38 N.
	7 16	☉ sets.
	9 30	Twilight ends.
	10 24·2	♃ 1 Sat. Ec. R.
19. M.	10 21	♃ 2 Sat. Oc. D.
20 Tu.	15 35	16 Taur. Oc. R. 187°
☾	16 7·7	
	16 10	20 Taur. Oc. R. 185
	16 14	19 Taur. Oc. R. 229
21. W.	8 18	♃ 2 Sat. Tr. E.
	9 37	♃ 2 Sat. Sh. E.
22. Th.	22 18·2	♀ tr. D. 19° 36′ N.
23. F.	1 29·4	☿ tr. D. 1 13 S.
	12 20	♃ 3 Sat. Tr. I.
24. S.	11 57	♃ 1 Sat. Tr. I.
	12 41	♃ 1 Sat. Sh. I.
25. S.	9 16	♃ 1 Sat. Oc. D.
	12 19·3	♃ 1 Sat. Ec. R.
	21 10·1	♀ ☌ ☾ ♀ 0° 9′ S.
26. M.	8 44	♃ 1 Sat. Tr. E.
	9 30	♃ 1 Sat. Sh. E.
27. Tu.	8 45·9	♃ 3 Sat. Ec. R.
●	17 59·6	
	9 38·9	♃ tr. D. 21° 10′ S.
	13 35·4	♈ transits.
	13 44·4	♄ tr. D. 1 46 S.
	22 23·6	♀ tr. D. 18 17 N.
28. W.	0 1·1	☉ tr. D. 9 41 N.
	1 11·7	☿ tr. D. 2 8 S.
Sept.	**G. M. T.**	**PHENOMENA.**
	h m	
	9 18	♃ 2 Sat. Sh. I.
	10 38	♃ 2 Sat. Tr. E.
	12 14	♃ 2 Sat. Sh. E.
	21	☾ Perigee.
Sept. 1. S.	10 53·6	♃ 4 Sat. Ec. D.
	11 3	♃ 1 Sat. Oc. D.
	22 28·7	♀ tr. D. 16° 45′ N.
2. M.	0 45·7	☿ tr. D. 1 44 S.
	8 11	♃ 1 Sat. Tr. I.
	9 5	♃ 1 Sat. Sh. I.
	10 31	♃ 1 Sat. Tr. E.
	11 25	♃ 1 Sat. Sh. E.
3. Tu.	8 43·3	♃ 1 Sat. Ec. R.
☽	8 25·7	
	9 8	♃ 3 Sat. Oc. R.
	9 18·1	♃ 3 Sat. Ec. D.
4. W.	10 3	♃ 2 Sat. Tr. I.
	11 55	♃ 2 Sat. Sh. I.
5. Th.	10 47	φ Sagitt. Oc. D. 133°
6. F.	7 31	*h*′ Sagitt. Oc. D. 99°
	8 58·7	♃ 2 Sat. Ec. R.
	7 10	♄ rises.
	8 56·8	♃ tr. D. 21° 18′ S.
	12 56·1	♈ transits.
	13 2·6	♄ tr. D. 2 4 S.
	13 4	♃ sets.
	15 11	♀ rises.
	17 24	☉ rises.
	22 33·4	♀ tr. D. 15° 0′ N.
7. S.	0 2·1	☉ tr. D. 6 2 N.
	0 12·1	☿ tr. D. 0 10 N.
	6 32	☉ sets.
	8 30	Twilight ends.
9. M.	9 59	♃ 1 Sat. Tr. I.
	11 0	♃ 1 Sat. Sh. I.
	11 33	♃ 4 Sat. Tr. I.
	12 2	7697 Oc.† 324°.
	19	☿ inf. ☌ ☉.
10. Tu.	7 19	♃ 1 Sat. Oc. D.
	9 4	♃ 3 Sat. Oc. D.
	10 38·6	♃ 1 Sat. Ec. R.
	14 8	7986 Oc. D. 120°
11. W.	7 49	♃ 1 Sat. Sh. E.
○	3 49·4	
	22 37·7	♀ tr. D. 13° 6′ N.
	23 36·1	☿ tr. D. 3 7 N.
13. F.	1	☾ Apogee.

† A near approach.

The positions of the Planets are given for intervals of 5 or 10 days, by their Mean Solar Times of Transit and Declinations. To find the Approximate R.A., subtract the G.M.T. of transit of ♈ from that of the Planet (increased by 24^h if necessary), and *add* $1^m·0$ for every 6^h of interval between the two times of transit if ♈ precedes, or *subtract* $1^m·0$ for every 6^h if the Planet precedes, to convert the interval of Mean Solar Time into Sidereal Time. The result is the Planet's R.A. at time of transit.

For the Occultations of Stars the position-angles of the points of disappearance or reappearance are given after the letters R or D. The angles are reckoned round the Moon's limb from *n* (true) in the direction *n, f, s, p* (*i. e.* from below towards the right hand, as seen in an inverting telescope). Stars from the British Association Catalogue are, for economy of space, denoted by their No. simply, the letters B.A.C being omitted. EDITOR.

Satellites of Saturn.

DISTANCES of Enceladus, Tethys, Dione, and Rhea from Saturn's polar diameter, expressed in semidiameters of the equator.

— means preceding or west, + following or east.

When the satellites are "N," or north of the ring, they move apparently from east to west; when they are "S," or south of the ring, they move from west to east.

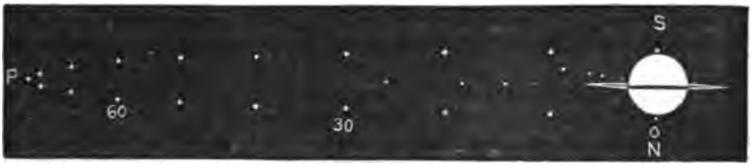
12^h G.M.T.	Encel.	Tethys.	Dione.	Rhea.
Aug. 15……	+0·5 N	+4·7 S	−5·3 S	−0·7 S
16……	+3·9 S	−5·0 N	+6·2 S	+8·6 S
17……	−1·4 S	+5·0 N	−2·9 N	+3·7 N
18……	−3·6 N	−4·8 S	−2·3 S	−7·3 N
19……	+2·3 N	+4·5 N	+6·0 N	−6·4 S
20……	+3·0 S	−4·1 S	−5·7 N	+4·0 S
21……	−3·1 S	+3·4 N	+1·5 S	+8·2 N
22……	−2·2 N	−2·7 S	+2·7 N	−2·1 N
23……	+3·6 N	+1·9 N	−6·3 S	−8·9 S
24……	+1·3 S	−1·0 S	+4·8 S	−1·0 S
25……	−4·0 S	+0·1 N	0·0 N	+8·5 S
26……	−0·3 N	+0·9 S	−4·8 S	+4·1 N
27……	+4·0 S	−1·8 N	+6·4 S	−7·1 N
28……	−0·7 S	+2·6 S	−3·7 N	−6·6 S
29……	−3·8 N	−3·4 N	−1·5 S	+4·8 S
30……	+1·7 N	+4·0 S	+5·6 N	+8·3 N
31……	+3·4 S	−4·5 N	−6·0 N	−1·8 N
Sept. 1……	−2·5 S	+4·8 S	+2·3 S	−8·9 S
2……	−2·8 N	−5·0 N	+2·9 N	−1·4 S
3……	+3·2 N	+5·0 N	−6·2 S	+8·4 S
4……	+2·0 S	−4·8 S	+5·3 S	+4·4 N
5……	−3·7 S	+4·4 N	−0·9 N	−6·9 N
6……	−1·1 N	−3·9 S	−4·2 S	−6·8 S
7……	+4·0 N	+3·3 N	+6·4 N	+4·5 S

12^h G.M.T.	Encel.	Tethys.	Dione.	Rhea.
Sept. 8......	0·0 S	−2·5 S	−4·3 N	+8·4 N
9......	−4·0 N	+1·6 N	−0·5 S	−1·5 N
10......	+1·0 N	−0·7 S	+5·2 N	−8·9 N
11......	+3·8 S	−0·2 N	−6·2 N	−1·7 S
12......	−1·9 S	+1·1 S	+3·1 S	+8·3 S
13......	−3·3 N	−2·0 N	+2·1 N	+4·7 N
14......	+2·7 N	+2·8 S	−5·9 S	−6·7 N
15......	+2·6 S	−3·6 N	+5·7 S	−7·0 S

Semiaxes of orbits expressed in semidiameter of Saturn's equator.

Sept. 2.	Mi.	En.	Te.	Di.	Rh.	Ti.	Iap.
Major	3·14	4·03	4·99	6·40	8·93	20·71	60·3
Minor	0·21	0·27	0·34	0·43	0·60	1·32	9·4
Hourly Motion..	15°·9	11°·0	8°·0	5°·5	3°·3	0°·9	0°·2

Iapetus reaches its greatest western elongation on Aug. 14, its superior conjunction north of the planet on Sept. 2, and its greatest eastern elongation on Sept. 21. A. M.



The diagram gives the positions of Titan for every 10° of longitude in his orbit from conjunction to elongation, and of the five inner satellites at elongation and at 20° from conjunction, showing the effect of foreshortening about the beginning of October. In September the rings and orbits are a little more open. By help of the diagram it will be easy to allow for the hourly motion in the orbit, and thus to find the approximate places of the satellites at any hour, from their positions as given in Mr. Marth's Ephemeris. The apparent orbits of all the satellites are of course similar to that of Titan.

Satellite of Neptune.

Semi-major and minor axes of apparent orbit 16″·7 and 5″·9, the angle of position of the major axis being 221°·6 or 41°·6. The satellite moves in the direction of decreasing position-angles from its "*n.f.*" elongation in position 41°·6 to its superior conjunction in position 311°·6, to its "*s.p.*" elongation in position 221·6, &c. The satellite is at or near its greatest elongation on

Aug. 16 8^h *s.p.*	Aug. 25 4^h *n.f.*	Sept. 2 23^h *s.p.*	Sept. 11 19^h *n.f.*
19 7 *n.f.*	28 2 *s.p.*	5 22 *n.f.*	14 17 *s.p.*
22 5 *s.p.*	31 1 *n.f.*	8 20 *s.p.*	17 16 *n.f.*

A. M.

Position of the Sun's North Pole.

1878.	Pos.-angle. °	′	Dist. from limb. °	′
Aug. 18	17	19	+6	52
23	18	53	7	2
28	20	20	7	10
Sept. 2	21	38	7	14
7	22	48	7	15
12	23	49	+7	13

The position-angle is reckoned from *n* (true) in the direction *n, f, s, p*; the distance is measured from the Sun's limb, in degrees of a Solar meridian, the sign + denoting that the North Pole falls within the limb or that the Earth's heliographic latitude is north.

The Moon.

1878.	Rises. h	m	Geoc. Libr. at midnight. long. °	lat. °	1878.	Sets. h	m	Geoc. Libr. at midnight. long. °	lat. °
Aug. 16...	8	7	−1·10	−5·56	Sept. 1...	7	55	+5·67	+6·76
17...	8	18	2·42	6·25	2...	8	25	6·62	6·48
18...	8	33	3·67	6·67	3...	9	6	7·17	5·84
19...	8	49	4·81	6·80	4...	9	59	7·31	4·89
20...	9	14	5·76	6·64	5...	11	5	7·10	3·71
21...	9	47	6·46	6·16	6...	12	19	6·58	2·38
22...	10	34	6·85	5·36	7...	13	35	5·80	+0·96
23...	11	35	6·86	4·27	8...	14	50	4·83	−0·48
24...	12	53	6·46	2·90	9...	16	2	3·72	1·88
25...	14	18	5·64	−1·32	10...			2·50	3·19
26...			4·42	+0·39	11...	Full.		+1·23	4·34
27...	New.		2·86	2·11	12...			−0·08	5·30
28...			−1·06	3·71	13...	Rises.		1·40	6·03
29...	Sets.		+0·84	5·06	14...	6	40	2·68	6·49
30...	7	16	2·68	6·05	15...	6	57	3·90	6·69
31...	7	33	+4·33	+6·62	16...	7	18	−5·92	−6·58

A. M

The Planets.

Mercury is an evening star till Sept. 9, when he is in inferior conjunction with the Sun.

Venus is a morning star, rising between 14ʰ and 15ʰ. Her disk is gibbous, the illuminated portion on Aug. 15 being 0·873, and on Sept. 15 0·936, and the corresponding diameters 11″·6 and 10″·6. She is occulted by the Moon on the morning of Aug. 26 (Aug. 25, 21ʰ G.M.T.), and the occultation *may* be seen at places within the limiting parallels of 52° N. and 27° S. latitude.

Jupiter is an evening star, retrograding through a small arc about 5° south of α and β Capricorni.

Saturn is visible all night, about midway between β Ceti and γ Pegasi. The following are the dimensions of the ring:—

	Outer Ring. Maj. Axis.	Outer Ring. Min. Axis.	Inner Ring. Maj. Axis.	Inner Ring. Min. Axis.	Ball. Diam.
Aug. 13	42″·77	3″·35	28″·44	2″·23	17″·2
Sept. 2	43″·63	2″·96	29″·01	1″·97	17″·5

The south side is visible, the elevation of the Earth above the plane of the rings being 4° 30′ S. and 3° 54′ S. on Aug. 13 and Sept. 2 respectively, and of the Sun 2° 48′ and 3° 6′ S. EDITOR.

THE OBSERVATORY,

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Researches on the Parallax of 61 (*A*) *Cygni.*

On the 22nd of August, 1868, my predecessor, Dr. Brünnow, commenced a series of observations on the difference of declination between 61 (B) Cygni and a star of the 9–10 mag. which follows in 51·5 secs. at a distance which was then 104″ to the north. These observations were repeated occasionally by Dr. Brünnow, and the last on record was made on 24th May, 1873. There are altogether twenty determinations of the difference of declination between the two dates I have mentioned.

After I had had some practice in the use of the South Equatorial, I recommenced the series of observations on the differences of declination between 61 Cygni and the following star which Dr. Brünnow employed. But there is an important difference between my observations as here recorded and those of Dr. Brünnow, to which I have already referred. In the latter case the following component of 61 Cygni was used, while in my observations the preceding component was employed. It will naturally be asked why I did not use the same component as Dr. Brünnow had done, and thus render the two sets of observations directly comparable. I must confess that, in the first instance, this was due to an inadvertence on my part. I was at first under the impression that Dr. Brünnow had used the preceding star, and under this impression I commenced my work. It was not until I had made a large number of observations that I became aware the component I was using was not the same as that which Dr. Brünnow employed; and when this discovery was made, the question arose as to the best course to be adopted. I was reluctant to discard the work I had done and recommence anew with the other component; and therefore I resolved to complete the series of measurements which I had commenced, and thus in the first instance to seek for the parallax from my own observations alone. At the same time I decided to commence as soon as possible another series of observations which would be strictly in continuation with Dr. Brünnow's work.

To this course I was led by some other considerations. In the researches of Struve on the parallax of this star he employed the following star, as Dr. Brünnow had done. It, however, appeared to me that on this very account a determination of the parallax in which the preceding star was used would be, if successful, of very considerable interest. I was also influenced by the reflection that as this was the first work of the kind in which I had engaged, I was glad to have an opportunity of the practice which it afforded before I commenced a series of measures which were to be amalgamated with those obtained by the mature skill of my predecessor.

I have to regret that, principally owing to the exceedingly bad weather which prevailed here last winter (1877–78), the number of observations is not so large as I would have wished. I have altogether 36 equations of condition for the determination of the four unknown quantities, on which the apparent difference of declinations depends. These contain the results of observations made on 35 different nights, which are only tolerably well distributed over the twelve months during which the observations extended. I have sought to increase the degree of confidence to which the result I have attained may be entitled, by taking an unusually large number of observations on each occasion. The method of observing which have used is almost identical with that employed by Dr. Brünnow in his researches on the parallax of 61 σ Draconis. A complete observation of the difference of declination is the mean of eight independent determinations.

The observations were first cleared from refraction by the application of the correction

$$+k\mathrm{D}\,\mathrm{cosec}^2(\delta+m),$$

where D is the apparent difference of declination, δ is the declination, k the coefficient of refraction given by Bessel, and m is taken from Bessel's Table, 'Astr. Unt.' Bd. i. p. 198, constructed for the latitude of Dunsink (53° 23′ 13″).

To clear the observations from the effects of precession, aberration, and nutation, and bring them to the date 1878·0, the following correction is applied:—

$$\begin{aligned}&+\overset{\prime\prime}{0}{\cdot}05266\,(1878-t)\\&+[6{\cdot}3089]\,i\\&-[6{\cdot}4137]\,h\cos(\mathrm{H}+\alpha)\\&+[7{\cdot}5735]\,g\sin(\mathrm{G}+\alpha)\\&+[7{\cdot}3643]\,h\sin(\mathrm{H}+\alpha),\end{aligned}$$

where i, h, g, H, G are the well-known constants in the Nautical Almanac.

In order at once to free the observations from the grosser part of the effects of proper motion, I have assumed that the comparison star is at rest, and that 61 (A) Cygni has the annual proper motion assigned to it by Argelander, viz.

$$+3''{\cdot}232.$$

When all these corrections have been applied, the following results are obtained :—

1877, July	3	66″·879	1877, Nov.	13	67″·344
	6	66·899	Dec.	13	67·701
	19	66·739		14	67·474
	22	66·927		17	67·482
	24	66·902		19	67·326
Aug.	4	66·798		29	67·350
	12	66·867		30	67·553
	31	66·959	1878, Jan.	31	67·659
Sept.	3	66·695	Mar.	24	67·273
	21	66·942		24	67·433
	27	66·901		31	67·252
Oct.	1	66·888	Apr.	1	67·001
	8	67·304		17	67·224
	16	67·013		24	67·161
	23	67·195		27	66·852
	25	67·021	May	18	66·851
	29	67·369		28	66·688
Nov.	2	67·300	June	1	67·341

Assuming that the true mean value of the difference of declinations is $67''\cdot150+x$, that the true relative proper motion of 61 (A) and the comparison star is $3''\cdot232+x'$, that ϖ is the annual parallax, and that k is the difference in the coefficients of aberration, then we have 36 equations of condition, which, being solved in the usual manner, give :—

$$
\begin{aligned}
x &= +\,0''\cdot0274 &&\pm 0''\cdot0210\\
x' &= -\,0\cdot0943 &&\pm 0\cdot1218\\
\varpi &= +\,0\cdot4654 &&\pm 0\cdot0497\\
k &= +\,0\cdot0330 &&\pm 0\cdot0493
\end{aligned}
$$

I subjoin the various determinations of the parallax of 61 Cygni which have been hitherto given (see Auwers, 'Abhandlungen der Akademie zu Berlin,' 1868) :—

Bessel, first fourteen months	0″·357
Bessel, last three months, and Schlüter	0·536
Johnson, first eleven months	0·526
Johnson, last seven months	0·192
Struve	0·511
Auwers	0·564

To these should be added the result obtained by Peters from zenith-distance observations—

Peters	0″·349

ROBERT S. BALL.

Binary Stars.—Part II.

GASPARIS computes the proportions between the radii vectores from the areas of the triangles between the distances in the apparent orbit. Four places would suffice for this; but he makes use of six in order to correct as far as possible the errors of the places read off on the interpolating curves.

Villarceau, the great French astronomer, has invented several methods for determination of double-star orbits. The most characteristic of them is formed in analogy with Laplace's method of determining the orbit of a body belonging to the Solar system from the three coordinates of a point in the orbit and their differential coefficients of the first order. For investigation of binary stars we require the angle and distance given for a certain epoch together with the differential coefficients of the angle up to the fifth order. The differential coefficients of the distance are easily obtained from those belonging to the angle, but the latter must be determined by Cauchy's formula for interpolation. The use of this formula is, however, objectionable; and Villarceau's researches on binary stars have not been much referred to outside of France.

Klinkerfues also determines at once the elements of the real orbit without a previous investigation of the apparent orbit. His first method starts from the polar equation of the ellipse. From five such equations two of the constants *a* and *b* are determined, and then $\frac{r_2}{r_1}$ and $\frac{r_3}{r_1}$ rom three equations. From these proportions, together with the angles and distances, the three radii vectores and node and inclination are obtained. When next the parameter is computed, *a* and *b* supply the excentricity and the longitude of the perihelion. The formulæ for finding *a* and *b* are very complicated. The method in this form supposes five complete observations to be available; but Klinkerfues has adapted it to the case where, besides three observations, the value of *k* is known. He then interpolates the distances ρ_4 and ρ_5 belonging to the angles $\theta_4=\frac{1}{2}(\theta_1+\theta_2)$ and $\theta_5=\frac{1}{2}(\theta_2+\theta_3)$ by aid of the formula given by Cotes for mechanical quadrature, and which is referred to in Gauss's 'Theoria Motus,' and he shows how to improve the accuracy of these distances by degrees so as to represent the given value of *k*. This latter way may be followed when the arc described is small.

If a greater part of the orbit has been described, and if the excentricity is not too large Thiele proposes to put $\theta_4=\theta_1+180°$ and $\theta_5=\theta_2+180°$; but he prefers to introduce a sixth distance and put

$$\theta_4=\theta_1+180°, \quad \theta_5=\theta_2+180°, \quad \theta_6=\theta_3+180°.$$

If *a* and *b* have been computed from Klinkerfues's formulæ, we find the corresponding distances by aid of the following equations; but if these distances are known with sufficient approximation we can find *a* and *b* from the same equations, in which latter case the third equation is a condition that must be satisfied:—

$$\frac{1}{\rho_4}=\frac{1}{\rho_1}+a\cos\theta_1+b\sin\theta_1,$$
$$\frac{1}{\rho_5}=\frac{1}{\rho_2}+a\cos\theta_2+b\sin\theta_2,$$
$$\frac{1}{\rho_6}=\frac{1}{\rho_3}+a\cos\theta_3+b\sin\theta_3,$$

We can then solve the following equations :—

$$(c+e)+(c-e)\cos 2\theta_1+d\sin 2\theta_1+\frac{2}{\rho_1\rho_4}=0,$$
$$(c+e)+(c-e)\cos 2\theta_2+d\sin 2\theta_2+\frac{2}{\rho_2\rho_5}=0,$$
$$(c+e)+(c-e)\cos 2\theta_3+d\sin 2\theta_3+\frac{2}{\rho_3\rho_6}=0.$$

We have now the constants of the equation of the apparent ellipse—

$$1+ax+by+cx^2+dxy+ey^2=0,$$

from which the elements of either the apparent or (directly) the real orbit are obtained.

In Klinkerfues's first method either the distances are supposed to be known for three or more epochs, or recourse is had to differential coefficients of the angles. It is, however, inconvenient to go through complicated calculations on data so uncertain, and it was therefore no doubt a great step forward when Klinkerfues discovered his second method, by which the elements of the real orbit are computed solely from the angles. Besides, the application of this method is easier than any other. As the apparent orbit is the orthogonal projection of the real orbit, the areas in one are proportional to those in the other. We have in consequence :—

$$\left.\begin{array}{l}\dfrac{\rho\rho_1\sin(\theta-\theta_1)}{\rho_1\rho_5\sin(\theta_5-\theta_1)}=\dfrac{rr_1\sin(v-v_1)}{r_1r_5\sin(v_5-v_1)}\\ \text{and}\\ \dfrac{\rho\rho_2\sin(\theta-\theta_2)}{\rho_2\rho_5\sin(\theta_5-\theta_2)}=\dfrac{rr_2\sin(v-v_2)}{r_2r_5\sin(v_5-v_2)}\end{array}\right\}\quad\ldots\quad(1)$$

From these we get by division :—

$$\frac{\sin(v_1-v)\sin(v_2-v_5)}{\sin(v_2-v)\sin(v_1-v_5)}=\frac{\sin(\theta_1-\theta)\sin(\theta_2-\theta_5)}{\sin(\theta_2-\theta)\sin(\theta_1-\theta_5)}\,.\,.\quad(2)$$

When six position-angles are known, we may find e, P, and T from three such equations by trial. When two complete observations and three angles besides are known, we find these elements from one equation like (2) and two of the same form as (1); and when three complete observations are given and one angle is known besides, we find them from one equation like (2) and two equations of the same form as (1).

When the inclination happens to be about ninety degrees, the orbit cannot be determined from six angles, because then all the observed angles are nearly equal to, or half a circumference different from, the longitude of the node. Klinkerfues has therefore adapted his third method to this case. It is convenient for calculation of the elements from three complete observations, and an angle of position or a distance, and only two quantities (the position of the perihelion and an excentric anomaly) have to be found by trial, instead of three as in the second method. The formulæ are developed by considering the oblique system of coordinates, whose axes are the projection of major and minor axes of the real orbit.

I have preferred to treat the case where the elements are to be determined from five epochs, in analogy with Klinkerfues's methods, as it is better to represent the epochs corresponding to the five position-angles exactly than to reject these for the sake of the observed distances. The proportion, $c = \frac{\rho_2}{\rho_1}$, of two distances for modern epochs complete the data required, when the axis major is subsequently computed. But often it is better not to represent this proportion exactly, in order to allow one or more of the three other distances to be approximately computed. From the tables referred to above, we obtain the function $f(E) = E - \sin E$ from the mean anomaly; and the equations (1) may then be written:—

$$\frac{f(E_1 - E_3) + M_1 - M_3}{f(E_3 - E_2) + M_3 - M_2} = c \frac{\sin(\theta_1 - \theta_3)}{\sin(\theta_3 - \theta_2)},$$

$$\frac{f(E_1 - E_4) + M_1 - M_4}{f(E_4 - E_2) + M_4 - M_2} = c \frac{\sin(\theta_1 - \theta_4)}{\sin(\theta_4 - \theta_2)},$$

$$\frac{f(E_1 - E_5) + M_1 - M_5}{f(E_5 - E_2) + M_5 - M_2} = c \frac{\sin(\theta_1 - \theta_5)}{\sin(\theta_5 - \theta_2)}.$$

The elements should be calculated with two different values of c, a near approximation to the value of which may often be gained from Dembowski's measures. The distances published by other observers will indicate how much to alter it to either side. When e, P, and T have been determined for c_1 and c_2, we can compute ρ_3, ρ_4, and ρ_5, without knowledge of the other elements, and their comparison with measured distances will serve as a guide in the interpolation between c_1 and c_2, that is between the two orbits:

$$\frac{\rho_3}{\rho_1} = \frac{\sin E_3 \sin(\theta_1 - A)}{\sin E_1 \sin(\theta_3 - A)},$$

$$\frac{\rho_4}{\rho_1} = \frac{\sin E_4 \sin(\theta_1 - A)}{\sin E_1 \sin(\theta_4 - A)},$$

$$\frac{\rho_5}{\rho_1} = \frac{\sin E_5 \sin(\theta_1 - A)}{\sin E_1 \sin(\theta_5 - A)}.$$

After computing the rectangular coordinates,

$$x=\rho \cos\theta \quad \text{and} \quad y=\rho \sin\theta,$$

we have

$$\tan A = \frac{y_1 \sin E_2 - y_2 \sin E_1}{x_1 \sin E_2 - x_2 \sin E_1},$$

$$\tan B = \frac{y_1(\cos E_2 - e) - y_2(\cos E_1 - e)}{x_1(\cos E_2 - e) - x_2(\cos E_1 - e)}.$$

When the three elements have been definitively determined, we find the remaining ones just as in Herschel's method:—

$$\tan \zeta = \tan v_1 \frac{\sin (B-\theta_1)}{\sin (\theta_1 - A)},$$

$$\tan (A+B-2\Omega) = \cos 2\zeta \tan (B-A),$$

$$\cos\gamma = \sqrt{-\tan (A-\Omega) \tan (B-\Omega)},$$

$$\tan\lambda = \sqrt{-\frac{\tan (A-\Omega)}{\tan (B-\Omega)}}.$$

It must, however, be kept in mind that the last-mentioned analytical processes presuppose a near approximation to have been attained by graphical methods; but when these have been successfully followed up, we may directly form equations of condition for correction of the elements. To correct all the elements simultaneously by differential formulæ is hardly ever possible, and even then troublesome. This method is occasionally followed when the investigator employs mechanical computers to carry out his directions, but it necessitates a number of repeated solutions of the equations. It is better to correct the different elements by degrees. Thus Klinkerfues's methods of correcting first the phoronomical and afterwards the geometrical elements have proved very efficient. I have found that the elements can be corrected in all cases with comparatively small trouble by variation of the epoch and period. Variation of the plane of the orbit is also useful in many cases, and gives occasion to a number of different processes, as I have explained elsewhere. That orbits may be corrected by variation of distances has been referred to above; but I shall here only explain the manner in which P and T are most simply varied. Of course any alteration of the value of the excentricity gives rise to more lengthy computation than alteration of the other elements, and it is therefore important to see that this alteration is carried out as simply as possible. With the approximate T, P, and e, I compute true anomalies corresponding to the epochs of the normal places, and obtain by the method of least squares corrections to be applied to the three other elements. With their new values I compute from the observed angles the true anomalies, and from these, with different values of the excentricity, the mean anomalies. From each of these different series of mean anomalies I next obtain T and P; and by a consideration of the outstanding errors of the mean anomalies, I interpolate a more correct value of e, and thence obtain T and P. The two latter elements are now successively altered, and with the last value of e three series of true anomalies are calculated, from each of which, by equations of condition, Ω, λ, and γ are found by the method of least squares. From the then outstanding errors the differential coefficients of the angles with respect to T and P are obtained, and these two elements found by equations of condition, and then Ω, λ, and γ by interpolation. Should the representation of the observations still be unsatisfactory, equations may easily be formed for correction of the three latter elements, together with the excentricity.

W. Doberck.

[To be continued.]

On the Daylight Correction of the Collimation of Reversible Instruments by the use of Distant Scales of Inches.

In equatorial telescopes, but especially in Newtonians, the adjustment of the reflecting surfaces frequently introduces a considerable and therefore inconvenient amount of collimation error.

In transit instruments the error is comparatively small, and solar or stellar observations are the usual means of correction, provided the clock rate is trustworthy. Of course N and S piers mounted with truly adjusted collimating telescopes are superior to all other means of correction; but these are not in general within the means of ordinary amateur observers. The Bohnenberger eyepiece is another auxiliary.

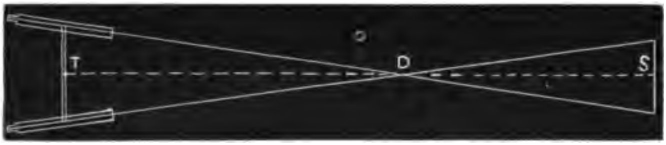
I propose here to describe a method which I believe to be new, and one I have had in use now for some time.

1. *The Equatorial adjustment of Collimation.*—The essential principle is, in the first place, the measurement of the *throw* of the instrument when the axis of vision is placed in the meridian, facing alternately east and west in the reversed positions.

The readiest method, though less accurate, of determining the throw T is to use a horizontal scale mounted in the observatory so that the positions of the centre of the object-glass in the two cases can be marked with a pencil, or by the use of a finely pointed plumb-bob, when by the hour-circle the instrument reads XII. and XXIV. In this case the declination axis can only be horizontal in the two positions, provided the polar axis is perfectly at right angles to the declination axis—an adjustment tested most readily when the collimation is corrected, by observing the pole star six hours before or after transit.

In my own 8-feet equatorial I measured the throw as 25·99 inches trigonometrically, and 26 inches by the mechanical use of plumb-lines or a fixed scale applied to the telescope ends in the E. and W. positions. As the whole thing depends upon an accurate measurement of the throw T, I shall now describe next the trigonometrical measurement of the throw.

I erected on a strong frame, D, well strutted, a plumb-line made of strong black linen thread (this should be well waxed with "heel-ball"). Another more distant erection, carrying an inch scale placed horizontally and at right angles to the line of sight, served to measure the parallax caused by the reversed positions of the instrument used to view the vertical thread at D. A diagram, perhaps, will render it clearer, the angle at D being very small.



Similar triangles with bases T and S are formed. Then if the scale S is parallel to the line of throw T, and the vertical thread D is distant 90 yards from T, and D 66 distant from S, the distant inch scale, we have—

$$66 : 90 :: S : T, \quad \text{or} \quad T = \tfrac{90}{66} S = \tfrac{15}{11} S.$$

It will be noticed that in this case the thread at D and the scale S, 66 yards further off, must be in the same field of view. In order to bring them both into distinct vision at the same instant the aperture of the object-glass must be contracted to a truly vertical one about half an inch broad. The exact readings of the scale by the black thread can then be taken to determine the throw T from the equation

$$T = \tfrac{15}{11} S = \frac{\text{Dist. of D from telescope}}{\text{Dist. of scale from D}} \times S.$$

This value will serve to correct the one formerly obtained.

Having thus obtained T, the observer now knows the exact distance along the scale upon which the axis of the telescope will be found to be displaced by being meridionally placed alternately in the reversed positions of the declination-circle facing east and west. Should the hour circle be depended upon and be made to read XII. and XXIV. alternately, the collimation will be adjusted provided on the distant scale the difference of the readings by means of a spider line be exactly the same as the ascertained *throw* of the instrument.

But if the axes of the declination and hour-circles be not perpendicular, a considerable error is here introduced by depending on the hour-circle. In that case I use a revolving level or apply a level to the uncovered declination bearings. But superior to this method is the use of two parallel scales of inches placed at S and D; and in this case much less distant scales can be employed.

Let S_1, S_2 be two parallel inch-divided scales at some distance, S_2 being at half the distance of S_1 we will suppose. Place the telescope on the most distant, with the hour-circle at XII., and by means of the spider line read both scales. Now reverse the instrument, and move the hour-circle until the spider line reads exactly the differential amount T; then, provided the vertical slit aperture be used, the near scale (without altering the focus) should read precisely the same differential amount. The parallelism of the two positions of the line of collimation will then be perfectly demonstrated, whilst the perpendicular error will be displayed by the reading XXIV.+ or −.

In one case I erected a beautiful brass scale by Harris divided into fiftieths of an inch, the divisions on which were distinctly visible at thirty yards distance. It is immaterial whether the near scale or the more distant one be first focussed upon. Practically I found two vertical black lines drawn on a white board exactly 26 inches apart a capital base of comparison. Two vertical threads of wire fixed on a frame separated by the interval T would be a more elegant apparatus.

2. *Collimation of Transit Instrument.*—On reversing this instrument in its Y's, there is possibly no appreciable displacement of the central position of the telescope, or, in other words, no throw to be measured. The test thread D, already described, however, at once sets the matter at rest. If there be no appreciable difference, then a very near scale of inches, and a vertical slit placed precisely central over the object-glass, will enable the observer to investigate the throw, if any, and its parallax depicted upon the distant scale by the method already described.

The throw or parallax thus obtained will serve to adjust the central spider line upon a scale not many yards distant, in the reversed position of the instrument, taking the mean reading.

It is plain that upon a heavenly body, or upon a very distant object, a small amount of throw or excentricity would be inappreciable; but when the instrument is used upon a near test object this must not be neglected.

When skies are thick with clouds and even sunshine obliterated, the regulation of the collimation and meridian of instruments by solidly fixed distant scales becomes an economical and consolatory use of dull weather. G. W. Royston-Pigott.

Double-star Measurement.

"The subject has already proved so extensive, and still promises so rich a harvest to those who are inclined to be diligent in the pursuit, that I cannot help inviting every lover of astronomy to join with me in observations that must inevitably lead to new discoveries."—*Sir William Herschel.*

The rich harvest of discoveries and valuable results foretold by Herschel in 1782 has been abundantly reaped. The great observer himself gathered no small share when his measures led him to the discovery of binary systems among the stars that gem the midnight sky. His distinguished son, South, Struve, Dawes, Smyth, O. Struve, Mädler, and others, who in turn took up this fascinating study, left behind them vast collections of measures of the systems Sir William watched with such pleasure and care. Still small indeed was the number of amateur astronomers who, at the close of the last century and during the first quarter of the present, could respond to the noble appeal of the father of double-star astronomy. Even H_2, South, Struve, Dawes, and Smyth, for many years the only systematic workers in the new field, were far from being splendidly equipped: the refractors used by H_2 and South had apertures of 3¾ and 5 in.; Dawes's first instrument was a Dollond refractor of 3¾ in., and that which for so many years did such excellent work in the hands of Dembowski was a dialyte by Plössl of 5 in. aperture. Moreover, not one of these observers had a driving-clock.

Great indeed is the contrast between the England of 1820 and of to-day in these respects. Equatorial refractors of from 4 to 6 or 7 inches aperture and reflectors of considerable power are now scattered broadcast in England and in the United States; vast collections of measures and a large number of orbits of binary stars are readily accessible; facilities for rapid intercommunication among the workers in any field of research are greatly increased; to say nothing of the stimulus given to the study by the noble devotion of wealth on the part of several private gentlemen to the establishment and maintenance of private observatories for the prosecution of double-star investigations. Nor must it be forgotten that several public observatories have found time to pay considerable attention to this subject: notably has this been the case at the Poulkova Observatory, where the distinguished director, son of the illustrious Struve, has for forty years carried on his double star-measures and investigations with eminent success. Mr. Ormond Stone, director of the Cincinnati Observatory, and his assistants have for some years been working with great spirit and care, especially among southern double stars. In the southern hemisphere, too, there are pleasing signs of activity.

Such considerations as these have induced us to invite amateurs who are desirous of doing some useful work to contribute their mite of observations to the rich treasures already gathered. How peculiarly double-star measurement is suited to the ordinary optical equipment and general circumstances of amateurs is sufficiently obvious. An equatorially mounted telescope of from 4 to 7 inches' aperture, provided with a fairly good micrometer and small brass setting-circles roughly divided—this is all that is necessary. A driving-clock is not at all necessary *; it does, of course, add very greatly to the comfort and ease of the observer, and in the case of very difficult objects it becomes almost necessary. Then as to the long night watches of the meridian and professional observer; of these the amateur, already weary with the labours of the day, need

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* On this point H_2 wrote in 1824:—"This, though rather difficult at first, becomes easy by a little practice; and even in unfavourable positions, the effect of the earth's diurnal motion may be almost exactly neutralized with a little management."

know little or nothing. All bright pairs are *best* and *most easily* measured in full sunlight or strong twilight; and there are many faint companions of bright stars which may be readily seen and measured soon after sunset with an aperture of from 7 to 9 inches. Lastly, this class of work does not require uninterrupted attention. Sets of good measures taken on any night have their full value independently of any future sets; indeed it is not good to take a large number of measures of any one object under ordinary conditions of atmosphere; it is better to take a moderate number on each of two or more nights.

And as regards books, Mr. Webb's 'Celestial Objects for Common Telescopes' and the 'Nautical Almanac' will supply the list of objects and general guidance which will be necessary for a good start. The subjoined list of important objects is offered as additional aid to the beginner.

The following short hints may be of service:—

1. Observe all bright stars in full day or strong twilight.
2. Attempt such objects only as are fairly within the optical power at command.
3. A few measures of any star on each of two average nights are better than many on one.
4. Take at least two double measures of distance and three of position.
5. Enter all observations fully at the time, adding remarks freely, and also a rough diagram in doubtful cases.

Any carefully made measures will be gladly received by the Editor, or by Mr. J. M. Wilson of the Rugby Observatory, or Mr. J. Gledhill of Mr. Edward Crossley's Observatory, Bermerside, Halifax.

LIST OF OBJECTS FOR SEPTEMBER.

Σ 2272 (H_1, II. 4; 70 Ophiuchi).

R.A. (1880) $17^h\,59^m{\cdot}4$, Dec. (1880) 2° 32′ N.

A very fine binary, usually called 70 *p* Ophiuchi. The *p* first appears in Flamsteed's MS. either by mistake or for some temporary purpose. Magnitudes 4·1, 6·1; colours, A yellow, B purple. The annual proper motion is $+0''{\cdot}216$ in R.A., and $+\,1''{\cdot}07$ in N.P.D.

M. Schur's elements are as follows:—$\tau = 1808{\cdot}79$, $\omega = 155^\circ{\cdot}7$, ☊ $= 125{\cdot}4$ (eq. 1850·0), $i = 57^\circ{\cdot}9$, $e = 0{\cdot}49149$, $\mu = -3^\circ{\cdot}8148$, $a = 4''{\cdot}704$. Period 94·37 years. (See 'Astr. Nachr.' vol. lxxi. 1868).

Encke's period was 73·76 years; Sir John Herschel's 80·34 years; Mädler's 80·61 years; Jacob's 87·52 years; Hind's 88·48 years.

Some recent measures:—

	°	″	
1873·51	88·8	3·89	Dembowski.
·57	88·6	4·01	Wilson and Seabroke.
1874·12	88·5	3·91	Gledhill.
·56	86·1	3·67	Dembowski.

	°	"	
1875·52	83·7	3·48	Dembowski.
1876·62	81·5	3·15	Dunér.
1877·52	77·6	3·47	Doberck.
·69	76·9	3·12	Dembowski.

OΣ 342.

R.A. (1880) 18^h 1^m·6, Dec. 9°·33′. N.

This is 72 Ophiuchi. It was measured by OΣ in 1842, by Mädler in 1847, and Secchi in 1857; but Dawes and OΣ failed to see the companion in 1848, 1850, and 1854; De., W. and S., Gl. and Sp. have examined it since 1860 with the same result.

Magnitudes 4, 8.

Σ 2281 (H_1, I. 87; 73 Ophiuchi).

R.A. (1880) 18^h 3^m·6, Dec. (1880) 3° 58′ N.

A binary system: very difficult in 1783; very slight change in the angle since 1841: the distance seems to be diminishing slowly.

Magnitudes 5·7, 7·2. Colours white.

Recent measures:—

	°	"	
1871·07	253·9	0·92	Dunér.
2·95	254·4	1·30	Wilson and Seabroke.
4·74	254·4	1·06	Gledhill.
6·61	250·0	1·03	„

Σ 2382=4 Lyræ.

R.A. (1880) 18^h 40^m·4, Dec. (1880) 39° 33′ N.

Magnitudes 4·6, 6·3. Colours, A greenish white; B bluish white.

Recent measures:—

	°	"	
1876·59	17·4	3·22	Wilson and Seabroke.
7·43	15·3	3·24	„

Σ 2383=5 Lyræ.

R.A. (1880) 18^h 40^m·4, Dec. (1880) 39° 29 N.

Magnitudes 4·9, 5·2. Colours very white.

Recent measures:—

	°	"	
1876·59	139·4	2·62	Wilson and Seabroke.
7·43	137·3	2·35	Doberck.

The common proper motion of this quaternary system is probably about −0^s·002 in R.A. and − 0″·09 in N.P.D.

Σ 2434 (H_1, IV. 127).

R.A. (1880) 18^h 56^m·6, Dec. (1880) 0° 53′ S.

AB. The motion is probably rectilinear. In BC we have a physical system in retrograde angular motion.

Magnitudes, A 7·9, B 8·4, C 10·3. Colours white.

		°	″	
AB.	1877·76	132·8	23·89	Flammarion.
BC.	1873·55	67·5	1·09	Wilson and Seabroke.

Σ 2579 (δ Cygni; H_1, I. 94).

R.A. (1880) $19^h\ 41^m{\cdot}2$, Dec. (1880) 44° 50′ N.

Discovered by H_1 in 1783; the companion became invisible to that eminent observer soon after, and was not seen again till 1826, when Σ detected it with ease. The companion reached its minimum distance about 1855 and was lost in the rays of the principal star. Hind and Behrmann have given the period as 178 and 280 years respectively. The formulæ deduced by Dr. Doberck are:—

$$\theta = 12^\circ{\cdot}48 - 1^\circ{\cdot}402\ (\tau - 1850) + 0^\circ{\cdot}0006\ (\tau - 1850)^2,$$
$$\rho = 1''{\cdot}64 - 0''{\cdot}0067\ (\tau - 1850).$$

Magnitudes 3, 8. Colours, A greenish, B ash. The magnitude of B has been variously estimated from the 7th to the 9th, and the colour also appears to be variable.

Recent measures:—

	°	″	
1875·02	335·3	1·60	Dembowski.
5·69	336·5	1·52	Dunér.
7·94	326·3	1·75	Dembowski.

Σ 2725.

R.A. (1880) $20^h\ 40^m{\cdot}6$, Dec. (1880) 15° 28′ N.

A binary; motion direct.

Magnitudes 7·3, 8. Colours, A white, B ash.

Recent measures:—

	°	″	
1877·82	0·9	4·8	Flammarion.
·69	358·6	5·0	Doberck.

Σ 2727 (γ Delphini; H_1, III. 10).

R.A. (1880) $20^h\ 41^m{\cdot}8$, Dec. (1880) 15° 42′ N.

A beautiful physical pair.

Magnitudes 4, 5. Colours, A orange, B bluish green; variable.

The common proper motion is $-\ 0^s{\cdot}004$ in R.A. and $+\ 0''{\cdot}19$ in N.P.D.

Recent measures:—

1877·82	270°·8	11″·25	Flammarion.

Subjoined are a few pairs, most of them in all probability physical, deserving of occasional careful measurement:—

Name.	R.A.	Dec.	Angle.	Dist.	Date.	Observers.	Mag.
	h m	° ′	°	″	1800+		
Σ 2289	18 4·8	+16 27	234·9	1·36	76·58	W. & S.	6, 7·1
Σ 2298	8·8	+41 21	178·4	...	5·62	„	8·5, 9·7
Σ 2303	13·6	− 8 3	227·0	[3″±]	58·60	Mo.	6·7, 9·2
Σ 2310	15·6	+22 45	237·7	5·24	74·85	Gl.	7, 10·3
Σ 2315	20·2	+27 19	247·0	0·38	5·90	Sp.	7, 8
OΣ 358	30·5	+16 54	202·1	1·73	6·59	„	6·8, 7·2
α Lyræ	32·9	+38 41	155·1	48·14	7·00	Fl.	1, 10·5
Σ 2384	38·4	+67 1	single		65·41	De.	8, 8·5
Σ 2437	56·6	+19 0	68·0	0·8	76·52	W. & S.	7·8, 8
Σ 2454	19 1·5	+30 15	232·4	0·8	·63	„	8, 9·2
Σ 2455	1·8	+21 59	104·8	3·5	5·61	Sp.	7·2, 8·3
Σ 2481	7·1	+38 36	225·6	3·9	6·75	Du.	8, 8
Σ 2576	41·0	+33 20	304·5	3·3	7·64	Fl.	7·8, 7·8
OΣ 387	44·3	+35 0	23·2	0·4	5·57	Sp.	7·2, 8·2
Σ 2596	48·5	+14 58	343·4	2·0	64·52	De.	7·2, 8·6
Σ 2603	48·6	+69 58	360·9	3·15	74·84	Gl.	4, 7·6

JOSEPH GLEDHILL.

Lockyer's 'Star-gazing'[*].

Book 1. The Pre-Telescopic Age.
2. The Telescope.
3. Time and Space-measures.
4. Modern Meridional Observations.
5. The Equatorial.
6. Astronomical Physics.

THE above table of contents is enough to show that the title of this book is very inadequate. "Star-gazing" is a proverbial expression akin to "wool-gathering;" but the book is better than its title.

There is a satirical photograph doing duty as frontispiece, hardly of happy augury, representing the most magnificent, but most idle, of British telescopes.

Probably a certain degree of childlike enthusiasm is necessary, or no astronomical work would ever be done. Astronomical operations are so protracted, and generally afford such ceaseless and depressing failures, that an extraordinary temperament is necessary to give any chance of success in them.

I imagine that few astronomers have worked harder than Mr. Lockyer; yet he retains something of the faith of a child in his instruments, and all a child's enchantment with their performances. He thinks they are truly wonderful, and expects you to think so too;

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[*] 'Star-gazing, Past and Present,' by J. Norman Lockyer, F.R.S. Macmillan and Co., 1878.

and he stands up to unfold their mysteries with all the airs of a magician.

My own impression is that, if you want to get a truly sickening sense of human fallibility, and of the hopelessness of human endeavour, you have only to make yourself thoroughly familiar with an astronomical instrument. To satisfy yourself on this point, and to escape the miseries that would fill your first few years of experience, you should read a little practical astronomy, such, for instance, as Mr. Gill on the Heliometer, in Lord Lindsay's Transit-of-Venus book*, or Captain Tupman on the photographs of the same transit.

What is called "popular astronomy" is of course a subject of the most attractive kind, and admirably adapted to make a selling-book; accordingly it has been tried by many writers who work chiefly with the scissors, and in our own day the crop of these books reminds one of the tares that the Devil sowed among the wheat.

There are writers who strut up and down the universe talking of "my Sun" and "my Moon" (meaning their own pitiful twaddle concerning those luminaries), as if all that is known about them was discovered exclusively by themselves, the fact all the while being that they could hardly see a haystack if it stood in their way, or, if they did see it, would not know whether it was not the great nebula in Orion. This is the stuff with which the "general reader" of our day is wont to feed his hungry soul in his "leisure hours." Mr. Lockyer's book is of quite another sort; its purpose and scope are to explain the structure and use of the instruments, machines, and appliances which have served to develop the science of astronomy, and on which our hopes of its progress depend. This is rather a large purpose, and requires a good store of knowledge: rather a wide scope, for it sets out to teach not only facts but methods of work and what tools are available.

The materials for the book are vast and magnificent, and these materials have long lain in a confused heap. We live in an age the most fertile in resource that the world has seen, and the inventions which now engross our attention threaten to become so complex that the beginner will have little prospect of getting to appreciate them unless the obscurity that surrounds their origin is cleared away for him by something like a consecutive history; and for this purpose we require a teacher who possesses two principal qualifications—practical knowledge and a habit of stating things very plainly,—so that matters which are already excessively complicated may not be hampered by a dry and difficult style of expression. Both these qualifications are possessed by the present writer in an unusual degree. The reader will not have to waste any of his time trying to make out what he means; and this I take to be a merit of no small magnitude.

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* Books of practical astronomy are always given away; I don't think anybody ever buys them.

There are, of course, plenty of small flaws to be found, and it will be my business presently to point out some of them, besides which I may be allowed to make one or two general reflections. It is unreasonable to suppose that this thick volume of information upon the most intricate and delicate results of mechanical ingenuity will be read by singularly ignorant persons—more ignorant, say, than a small school-board pupil; and yet the writer often addresses himself to such. It would almost seem to be his habit to assume that his audience consists exclusively of born fools; at all events, he provides against such a contingency. He does not seem aware that the simple laws of optics are as universally known as the simplest rules of arithmetic; so that a good deal of the nice paper and beautiful printing of the volume are unnecessarily squandered. In one place he speaks of a particular "sort of curve called a parabola"!

I will make another general remark: one of the uses of an elementary book is to interest the reader in the subject. This end the book amply serves; but there is another use which, I regret to say, it serves not at all, and that is, to inform the reader upon the sources of further information. The absence of all reference to authorities I take to be the most serious defect of the book, so that as an introduction to more serious study it is quite useless. The author says occasionally (as at p. 361) that he "must refer the reader to other works on the subject," but he never does so; he hardly ever mentions another "work" of any sort. You are served with a spoonful of information, but never told where the tureen is from whence he ladled it.

The best evidence of success in teaching is, that the pupil asks for more: here he will ask in vain.

Now I will give a short summary of what the reader *will* find. If he does not attempt to read the book at all, he will find a very handsome guinea's worth in the pictures. It is the best illustrated book that I remember to have seen. The woodcuts are very numerous, and nearly all of them are, in their way, above the average standard of quality.

In his first chapter the author sketches the early history of astronomy in an easy and pleasant fashion. This does not present any considerable difficulty, nor exhibit any particular skill; but it serves sufficiently well to lead on the beginner to take an interest in the more original and special teaching of Book 2. "In the old time," he says, "the only instrument was the horizon; and, truth to tell, in a land of extended plains and isolated hills, it was not a bad one. Hence it was, doubtless, that observations in the first instance were limited to certain occurrences, such as the risings and settings of the stars, and the relative apparent distances of the heavenly bodies from each other." The Pyramid, he says, was the next astronomical instrument. In chapter 2 he describes the Gnomon as the first meridian instrument; and so on.

In the next few pages Hipparchus and Ptolemy pass across the field of view, and practical astronomy begins to interest the reader.

Whenever Mr. Lockyer speaks of an instrument or the inventor of it, he reminds one of the charm of one's early studies, when one continually read and re-read without weariness every old book and paper that told of the half real, half ghostly figures of early astronomical history and the dawn of instrumental operations. The figure of Tycho Brahe is one of the most fascinating of these; and hereabouts, with the invention of the clock and the transit-circle, he concludes his "pre-telescopic age." In the next chapter the laws of refraction are elaborately set forth; and further on, where the growth of the telescope is treated of, this amplification is well bestowed, since few but practical opticians are likely to find it superfluous.

The interest of the subject increases as its subtleties are unfolded, and I have nothing but admiration for this part of the book. The description of the achromatic refractor is both clear and compact, and is nearly all that could be wished; but I think it regretable that the treatment of spherical aberration is omitted in this place, and only explained at a much later stage.

In his chapter on the reflecting telescope the author reproduces, as has always been done in books on optics, what is called the "front view" reflector on equal terms with the feasible forms of the instrument. It is a pity to perpetuate the memory of this ridiculous contrivance and allow the innocent beginner to associate it with the great name of Herschel. A telescope in which nothing can possibly be focussed must be a truly valuable and delightful instrument! but it teaches one useful lesson—it shows how indiscriminately the follies and the achievements of a man are cherished by his followers. You do not expect a book on the telescope to be original; but a practical observer like Mr. Lockyer might leave off copying out such rubbish.

The chapter on eyepieces is most useful. That beautiful invention, the Huyghenian eyepiece, is explained and illustrated with remarkable clearness. One or two solar eyepieces are described, but their names, or rather those of their meritorious inventors, are unaccountably omitted. The solar eyepiece of Dawes is described, but the improvement on it by Dr. Erck is omitted; and the most important of them all, the polarizing eyepiece, is barely hinted at. The author enters with great "gusto" into the subject of object-glasses, and collects all the interesting facts I have ever heard on the subject; and an admirable chapter they make. The competitions of the English, the Americans, and the French, and the story of our own factory (Mr. Chance's) accommodating them all, afford amusing reading. Mr. Lockyer says the process of manufacture is a secret, and at once goes on to tell all about it. The metallic speculum is rather neglected (I may say "scamped"), and odd things are told about casting. The grinding and polishing of specula is a consummate mechanical operation, and no one without experience can speak of it without missing the subtleties on which its interest depends. No one but a practical experimenter is likely to read such a chapter at all, and he will not fail to notice the absence of the true mechanical faculty.

In describing machines used in this process, one of Mr. Lassell's is prominently illustrated, which has been superseded by a later contrivance of that distinguished mechanic; but no reference is made to the paper in which his modifications are described. Mr. Lockyer says, the polisher "takes a very devious path;" and perhaps the writer will be thought to have done so in several of these pages; but when he comes to the manipulation of the finished instrument, no man is more thoroughly in his native element. He has a passion for an instrument, and, in the words of an ancient writer, may be said to "roll it as a sweet morsel under his tongue," or, in modern terms, to describe it very felicitously.

The faculties of the telescope are admirably dealt with—the relation between magnification and aperture, and between illumination and focal length, and the capabilities of eyepieces especially so; but the omission of the term "*intrinsic* brightness" at p. 157 allows a confusion in the reader's mind between brightness and quantity of light. The comparative merits of refractors and reflectors are carefully considered and with every wish to be fair, but a trace of prejudice against the latter is apparent; one of its conspicuous merits—the horizontal view, for instance—is not mentioned.

On the whole, probably the most interesting and useful chapter in the book is that on the Equatoreal. The various forms of the instrument in use are illustrated by engravings and their peculiarities pointed out. The reader will see with interest the recent great American telescope, which has already achieved brilliant results; but I think he will be rather astonished at the apparently slight and inadequate character of its mounting as shown in the woodcut.

The adjustments of the equatoreal are described more clearly than by any other writer, so far as I know; and many an amateur will be grateful for this clearness; but, in common with the other writers, he omits any reference to a difficulty which I suppose is always encountered in setting up an equatoreal, that of eliminating the error of the polar axis, east or west. In Cooke's mounting, at all events, no provision is made for correcting it, and you have to heave the whole instrument round by direct manual force, the effort required to move it always carrying you too far on the other side. I do not understand why this barbarism should remain in such beautiful instruments. It would cost only a trifling expense to grind true the surfaces of the azimuth flange in the middle of the pillar, and fit them with a tangent screw. In his example the author finds an error of the polar axis of 1′ 38″ east. I hope he tried to correct it, and enjoyed the experiment. The chapter on the ordinary work of the equatoreal will be much valued, especially that part of it on the micrometer, with which I know of nothing to compare for usefulness to the amateur usefulness, in short, is the strong point of the book, and it will save many a star-gazer from groping in the dark for years.

As a literary performance I am rather disappointed with it, and chiefly because of its lack of finish. Loose forms of expression are scattered broadcast, where accuracy would be both easy and desirable. A few instances will show that these blemishes are the effect of hurry and want of careful revision. At p. 1 he says, "so coeval was it," meaning so nearly coeval; at p. 27, "this plane or starting point;" at p. 29, "a plane at right angles to certain points;" at p. 62, "principal focal length," for principal focus; at p. 90, "at *that* angle will it be reflected," meaning at an equal angle; at p. 109, "a simple double convex lens," meaning a solitary double convex lens; at p. 150, "move the diagonal mirror by means of the *motions* which are provided," meaning the screws which are provided; at p. 233 he speaks of the astronomer's "stock in trade," meaning his tools; at p. 376 he talks of the "blue end of the spectrum," meaning the violet end.

I do not think that such blemishes should be called trivial, especially in a scientific book, for they irritate the reader immeasurably if he does not happen to be a slovenly reader; and a slovenly reader ought not to be put to a scientific book at all, but to feed out of his proper trough. The explanation in this case is, that Mr. Lockyer's business is rather to make the science of astronomy than to write about it; and every hour that he takes from his legitimate business is a loss to the world. Still I am very thankful to him for his book, and I am sure that multitudes of other people will be so too. JOHN BRETT.

CORRESPONDENCE.

To the Editor of 'The Observatory.'

The Transit of Mercury, 1878, *May* 6.

SIR,—

In the number for April you made several suggestions to observers relative to observing the Transit of Mercury. Having had little experience in such work, I beg leave to thank you for them, and to send you some account of my observations. I am the more willing to do this, since we were very favourably situated, having the middle of the transit nearly at noon.

We had the favour of a splendid day—all that could be desired. As careful and complete preparation was made as my resources would allow. The aperture of our 12¼-inch equatorial was reduced to 7 inches. Special care was taken to insure a good focus. To make sure of opportunity for this, several nights in advance the instrument was turned on various objects, as Castor, γ Leonis, Uranus, and finally on the Satellites of Uranus. I used, as you suggested, as high a power as I thought safe. The micrometer eyepiece employed was that generally used, except in separating and measuring double stars whose distance is less than 3″. Its amplifying power is estimated to be 275. A shade of blue glass was screwed over the eyepiece. The time was carefully corrected from transits of standard clock-stars, observed on our 6-inch meridian circle, by Troughton and Simms. Our latitude is well determined from transits of circumpolar stars. Our longitude from Washington is not yet determined by telegraphic interchange of clock-signals; it will be determined soon. We use for latitude and longitude the following values:—

Latitude...... 39° 16′ 16″·75 ± ·05 North.
Longitude.... 6^h 11^m 21^s West from Greenwich.

Our altitude is 700 feet above the level of the Gulf of Mexico.

Fifteen minutes before the computed time of ingress the instrument, as previously adjusted, was turned on the Sun, the driving-clock set to work, the telescope clamped, and the position-circle and threads properly placed. I had abundant time and rare opportunity to search for the planet outside the Sun. With my utmost scrutiny I could detect no trace of the outline of the planet projected on the solar corona, nor could I discern any halo. The edge of the Sun's disk, with the exception of a few notches or scratches, was clearly defined and remarkably steady. Indeed I was surprised at the absence of glare and tremor.

The protruding segment of Mercury, when first seen, was in contact with my thread; so that my eye had been directed to the *proper point* on the Sun's border. No arc of light nor any halo could be detected. The notch or protruding segment was recognized at 21^h 1^m 54^s, Glasgow mean time. From observation of a *similar segment* at egress, I estimate external contact to have occurred 8 seconds previously. Hence I note *true external contact* at 21^h 1^m 46^s. The phenomenon of the cusps, as indicated in Prof. Newcomb's diagram, was noted at 21^h 4^m 4^s, which is a mean of several comparisons. The cusps continued very steady and distinct until very near the internal contact; then there was a slight undulation of light, but far less than I had expected. I ran my eye rapidly along the curves of the approaching cusps, disregarding the undulations of light, and sought to note the time when the directions of these curves met on the edge of the Sun. This instant I took for the *true internal contact*—occurring at 21^h 4^m 34^s. All the times were noted by the chronograph. The shading of the eyepiece was such as to give the Sun a bright blue tinge, free from all glare; and the impression on my eye was, that it was not more than two or three times as bright as a perfectly clear sky illuminated by the Sun when two hours above the western horizon. Subsequent to internal contact, and following only a few seconds later, was a dark band or ligament (as a drop of fluid) connecting the points of contact. There is no use of cavilling at this expression. It was a real phenomenon. It was not *pointed* or *pear-shaped*, but an apparent dark ligament gradually growing brighter. At first, even after contact was noted, it seemed bounded by two curves slightly convex to the *line of contact*; afterwards there was a *single curve* protruding toward the Sun's edge. I noted the time of the *middle* of this phenomenon at $2^h\ 4^m\ 43^s$, which is a *mean* of several comparisons; and at $21^h\ 4^m\ 54^s$ separation was *beyond question*, and a clear band of light showed between the disks. At the moment of separation the *outmost* point of the dark curve seemed in contact with the Sun's edge.

As soon as Mercury was fairly on the Sun I again sought for the arc of light and the bright spots. My finder of three inches aperture was well adjusted, and furnished with a shade very similar to that on the large telescope. In the finder a halo, two or three seconds in width, was plainly visible during the whole transit; but in the large instrument no such appearance was detected, nor any spots of unusual brightness on the disk of the planet. Again and again, during the seven and a half hours of transit, and with the clearest sky, did I ply every power of vision to detect these, but without success. I also accommodated more than 50 visitors during the intervals of observation, and I invoked *their aid* to detect them, but in no instance with success. The halo in the small telescope, however, was obvious to every observer. The *blackness* of Mercury on the Sun's face was relieved by a *bluish tint*, and the image was very clearly defined. While no spots of *unusual brightness* were detected, I am sure the bluish-black disk of Mercury was not *uniformly* dark. There were *alternations* of light and shade—a slightly *mottled* appearance. I think this appearance was most striking about 3 P.M. in a region of the disk a little S.W. of the centre. The mottled appearance recalled to mind the varying degrees of light and shade in the central part of the nebula of Orion; it simply *suggested* this, but was in no degree *like it*.

During the progress of the transit I made 29 double measures of the diameter of Mercury—nine in two series from north to south, nine in two series from east to west, six in two series from N.E. to S.W., and five in one series from N.W. to S.E. I did not expect them to be very accordant with each other, much less to have any considerable weight in the question of the compression of Mercury. The temperature of the screw was variable and excessive. Still I was prompted to make these measures from the fact that Mr. W. R. Dawes had deduced from measures made during the Transit of 1848, for the compression, the value $\frac{1}{29}$. I am more than ever satisfied that a *reliable value* for the compression of Mercury cannot be deduced from micrometer measures during a transit. Of *one thing*, however, I feel *well assured*—the apparent diameter of Mercury, June 5–6, was perceptibly *less* than that assigned in the ephemeris, by the $\frac{1}{24}$th or $\frac{1}{20}$th part. All my measures are *uniform* in this testimony, and they are confirmed by the measures of Prof. Boss of the Dudley Observatory, Albany, N.Y.:—

North to south,	mean of	9	measures in	two	series	11″·419
East to west	„		„	„	„	11″·405
North-east to south-west,	mean of	6	meas. in	two	„	11″·389
North-west to south-east	„	5	„	one	„	11 ·300

The small differences in these series, I am sure, are not due to any real difference in the apparent diameters. The compression is *too small* to be detected by such measures. Half an hour before the egress I noticed one phenomenon with much satisfaction. It was the globular form of Mercury, most clearly revealed. Instead of a flat disk there was a spherical surface, from which rays of light seemed to glance off. The appearance resembled the *raised convexity* of the Moon's bright surface when the terminator is well advanced. It showed *conspicuously* on the side next the point of egress.

There was one phenomenon over which I was puzzled, and I mention it, hoping it will receive some attention from those conversant with the theory of light. About an hour before the egress I found that whenever the micrometer-thread was brought near the edge of the planet, on the preceding side, there was invariably a *seeming protrusion* of the planet towards the thread. The appearance was analogous to the "dark ligament" attending internal contact, though greatly diminished in extent. I am sure of no deception in the matter; for, entirely unlooked for, it obtruded itself, so as to *compel* my attention, and I repeated the experiment again and again. Whenever the thread, tipped with sunlight, was brought near the edge of Mercury, invariably this apparent protrusion occurred. An illuminated edge seemed *essential* to it. Mr. Boss of Albany, who observed with a 13-inch equatorial, reduced to 8 inches, also saw the same appearance, and says it gave him much annoyance. My measures had all been *completed*, hence it gave me no interruption.

The phases at egress were noted as follows:—

Dark band or ligament (mean of several comparisons)........	$4^{h}\ 30^{m}\ 58^{s}$	Gl. M. T.
Second internal contact........	$4^{h}\ 31^{m}\ \ 5^{s}$	„
Second external contact	$4^{h}\ 33^{m}\ 52^{s}$	„

As at the ingress, the images were very steady and the outlines remarkably well defined. The conditions could not have been more favourable. Of the last external contact, I *must say* that it cannot possibly be one half second in error. In the other phases there was *room for difference of judgment*; but in this case the vanishing segment of Mercury was watched, and seen to be extinguished as suddenly as a star to disappear on the Moon's ashy limb. On any other phase two observers at the same telescope might *honestly differ* by *one* or even *two* seconds. In this phase such a difference was *impossible.*

The search for the planet outside the Sun's disk was again unsuccessful, nor could any arc of light be seen. The whole time of transit was $7^h\ 32^m\ 6^s$, which, so far as I know, is the longest on record. I am, Sir, yours very respectfully,

Morrison Observatory, Glasgow,
Missouri, U.S.A.

C. W. PRITCHETT.

The Eclipse of the Moon, 1878, *Aug.* 12.

SIR,—

Last night the Lunar Eclipse was observed here. After a rough and stormy day the Moon rose bright and clear, and promised well for observation. By the time, however, of first contact of the umbra, fleecy clouds and mist were coming up from the S.W., and these more or less obscured the Moon's disk until our leaving the observatory after the middle of the eclipse. At one time only was the partially eclipsed Moon clearly defined. The Earth's shadow was then observed to be pure grey in tint, like a thin wash of lamp-black. No rosy copper or green tint was remarked. A fine annular halo was noticed at about 11 o'clock surrounding the Moon at a considerable distance. About the time of greatest obscuration, the portions of the sky which were visible through the clouds became of a deep blue, and the stars shone brightly; but the loss of light was not very marked. On the 7th instant, I was testing a new 6-inch object-glass by Cooke, of York, on Jupiter, and, considering the planet's inconsiderable altitude, I was much struck by the sharpness and beauty of its details. Satellite IV. and its shadow were on the disk at the time. The satellite which was near the planet's limb appeared to me of a light brown-grey tint, and with a somewhat coarse nebulous outline. The shadow was intensely black and sharp. The belt and polar tints were very distinct. The rose and cream-ochre colours of the former were at once remarked by an observer of Jupiter for the first time, who was present.

It seems to me that something may be done with Jupiter at the present time, even with his present low position, if intervals of air stillness be watched for and taken advantage of. I have frequently examined Jupiter when well up in the sky without so favourable a result as that of the other night; and I concluded (after allowing for the fine quality of the glass) there must have been something peculiar in the state of either our own or Jupiter's atmosphere to account for the unusual bright colouring and sharpness of detail which I noticed. I am, Sir, yours faithfully,

Guildown, Guildford,
13th August, 1878.

J. RAND CAPRON.

The August Meteors.

SIR,—

Observations of the Perseids this year were rather difficult to obtain, owing to the moon and cloudy weather; but on the night of the 10th, 113 meteors were seen, in 4 hours, between 11.30

and 3.30, more than half the time being in strong moonlight, and the rest in partial twilight.

Of this number, 97 were Perseids, about 60 of which had visible streaks, and 20 were coloured.

The horary number was 28, taking the night through, 44 being seen in the hour 2.15 to 3.15.

The radiant was distinctly marked at 43° +56°.

The finest meteor appeared at 2.28, but I only saw the flash and streak. As I was mapping, with a bright lantern light on the paper, the meteor must have much exceeded Venus in lustre to have called my attention to it.

About 1 o'clock several foreshortened meteors were seen near the radiant, giving a point about 45° +57°, one being absolutely stationary at 47° +58°.

A few days previously several meteors gave a radiant in Perseus at 43° +55°, very nearly at the position of the principal number of tracks seen on the 10th of last year. I was unable to prove the shifting of the radiant on the 11th, the evening being overcast.

There was a great falling off in numbers on the 12th, only a few Perseids being seen, and these agreed with the position given above.

I am, Sir, yours truly,

H. CORDER.

Writtle, Chelmsford.
1878, August 20.

Supposed Discovery of Vulcan.

SIR,—

During the total phase of the late solar eclipse, as observed at Denver, Colorado, I had a view of a celestial object not down in Argelander's charts, which to my mind, without any doubt, is the long-sought Vulcan.

It was in field with a star of the same magnitude, probably θ Cancri.

As soon as totality was over, I recorded in my notebook the following:—"Saw two stars, about 3° S.W. of Sun, apparently of the 5th mag., some 12′ apart, pointing towards Sun; both red." I immediately acquainted Prof. Hough, formerly director of the Dudley Observatory, and my two assistants of the discovery, and, as soon as possible on the same evening, Mr. Burnham, the celebrated double-star observer, and many others. None of them knew of any star in that position answering the description.

On my way home the thought occurred to me that the distance between them was about equal, perhaps a little greater than half that between Mizar and Alcor. On arriving at home, I found Webb gives their distance as 11½′, which would make the distance between θ Cancri and the new object some 6′ or 7′ instead of 12′, as estimated (hastily and roughly, of course) at the time.

The next morning I learned by telegraph that Prof. Watson, at Separation, Wyoming Territory, saw a star 2½° S.W. of the Sun and of 4½ mag., not down in any chart.

The two observations are therefore confirmatory each of the other.

As totality commenced some four minutes earlier with him than with me, of course he antedates me by that amount.

Telescope 4½-in. aperture; comet eyepiece; power 25; field 1½°; periscopic and very superior.

I am, Sir, yours truly,

Rochester, N. Y., Aug. 13, 1878. Lewis Swift.

NOTES.

The Eclipse of the Sun, 1878, July 29.—News has been received of the success of all the parties stationed at various places along the line of totality in the neighbourhood of the Rocky Mountains, and probably no eclipse has ever been so completely observed. Instructions for observers were drawn up with great care by Prof. Harkness and issued by the U. S. Naval Observatory; and the American Astronomers threw themselves with such zeal into the task of organizing expeditions, that the whole district was sprinkled with observers, stationed at various altitudes, from Pike's Peak, 14000 feet above the sea, to the level of the plain. From the accounts received so far, it appears that the corona, in correspondence with Sun-spots, prominences, and other manifestations of solar activity, was small and comparatively faint, though even in this eclipse it extended on an average to a distance of 26′, corresponding to 700,000 miles from the Sun. Beyond this Prof. Newcomb, with the assistance of a screen on a high pole, traced what he considers to be the Zodiacal light on each side of the Sun, along the ecliptic, to a distance of 6°—a most important observation. Prof. Cleveland Abbe, at a height of 10,000 feet on Pike's Peak, saw two pairs of streamers (one in the ecliptic and the other at right angles) looking like two wind-vanes, one end of each pair being pointed and the other, on the opposite side of the Sun, fan-shaped. These he considers to be meteor-streams seen in perspective. The true or inner corona formed a ring less than 5′ broad and as bright as the full moon.

Another very important result is, that the corona in this eclipse gave a continuous spectrum without any of the bright lines which have been seen on former occasions. The famous 1474 K line was now conspicuous by its absence. This fact was established not only by Mr. Lockyer's eye observations, but also by photographs taken by Dr. Draper, Prof. Harkness, and Mr. Lockyer, a diffraction-grating having been in each case placed in front of the camera. We trust that the success of this method will be a lesson to certain Fellows of the Royal Astronomical Society, who with glib assurance asserted that it was physically impossible to photograph the spectrum of the corona, and who did their best to prevent the method from being even tried under Mr. Lockyer's auspices, in the eclipse at Siam. The possibility has now been proved by three astronomers, in the case of a comparatively faint corona giving a continuous spectrum, which is a condition much less favourable than a spectrum of two or three bright lines.

The heat of the corona has also been the subject of observation, having been measured by Prof. Edison with his new tasimeter (an instrument in which the expansion of a strip of vulcanite under heat compresses a carbon button, and by thus diminishing its electrical resistance deflects a galvanometer needle) and by Prof. Young with a thermopile. The latter found a heat-line in the ultra-red from the corona, but none in the ultra-violet.

But the great achievement was the discovery by Prof. Watson and Mr. Swift of what appears to be the long-sought intra-Mercurial planet. In a letter to the Astronomer-Royal, Prof. Watson, well known as the discoverer of so many minor planets, announces that he found, during the eclipse, a ruddy star of 4¼ mag., which, with a power of 45, showed a perceptible disk, without any appearance of elongation such as might be expected if it were a comet. Its position was at 5^h 16^m Washington M.T., R.A. 8^h 26^m 54^s, Dec. 18° 16′ N., having been referred to the Sun by means of pencil-marks on cardboard disks, placed on the circles of the equatorial to expedite the record and avoid all risk of mistake. The new star was very much brighter than θ Cancri, which was a little further west (R.A. 8^h 24^m 40^s, Dec. 18° 30′ 20″ N.).

Swift's Comet.—From a circular of the Vienna Academy, it appears that four observations have been obtained by Prof. Peters at Clinton of the comet discovered by Mr. Lewis Swift on the morning of July 7; and from these Dr. Holetschek has computed the elements as follows:—

Per. Pass.	1878, July 21·2626 Berlin M. T	
ϖ	280° 19′ 51″	Mean Equinox 1878·0.
☊	102 18 23	
i	78 0 50	
$\log q$	0·14328	

The comet was not observed at any European observatory, although carefully searched for—a circumstance which is probably due to a misunderstanding as to the date of discovery, which was taken as July 7 14^h astronomical reckoning instead of July 7 2^h A.M. civil reckoning, besides which the comet was faint and had a *rapid* motion south instead of a *slow* motion south-west, as stated in the telegram. The comet soon passed out of view for northern observatories, its declination on July 23 being 21° south, with a motion southwards of about 2° a day. The brightness also fell off rapidly, being only 0·37 on Aug. 8, and 0·13 on Sept. 1, according to Dr. Holetschek's ephemeris, the brightness at the time of discovery being taken as unity.

Meteor Notes for September.

This is a month for which we require many further observations. In August and October a large number have already accumulated (owing to the occurrence of periodical showers in those months); but September appears to have been much neglected, though it offers equal inducements to the observer. There is an active display of swift streak-leaving meteors from 61° +37° on about Sept. 7–16, seen by the writer last year, and formerly by S. and Z. and Tupman. The latter also recorded 14 similar meteors from a point at 65° +6° on Sept. 13–15, 1869; and another strong shower was seen on the few nights preceding at 78° +23°, Sept. 8–10. S. and Z. give a radiant at 28° +35° for Sept. 23. These should be looked for again; also several morning showers traced by the writer last year on Sept. 15–16 as follows:—156°+41°, 130°+46°, 101°+11°, 88° +17°, 87° +34°, and 73° +14°. These observations A.M. may well be made (in the absence of moonlight) during the first and last seven days of the month.

Recent Observations.—At Bristol, during the 8 nights from July 26 to August 2, 403 shooting-stars were seen in 22 hours of watching. On July 27–30 a very rich shower was noted from near δ Aquarii at 341° −13° (54 meteors). They were rather slow, with long paths of about 17°, and quite devoid of streaks. The same shower was accurately seen by Capt. Tupman at 340° −14°, on July 27, 1870, and the few ensuing nights (see No. 43 of his Catalogue), when he recorded about 63 meteors from it (July 27–Aug. 1). This stream evidently reaches a strong maximum. On July 31 this year it had become extremely feeble. Another specially prominent shower was observed chiefly on July 30, 31, and Aug. 1, from 3° S. of the star-cluster χ Persei, or at 32° +53° (63 meteors); 44 meteors of this system were seen on the three nights mentioned. They were very swift and short (averaging 7°), invariably leaving streaks of about 3° or 4°, and in strong contrast to the long, gradual paths of the *Aquarids* traced on the few nights preceding. This shower below χ Persei is quite distinct from the August *Perseids*, and gives an earlier display of marked intensity. It agrees well with the radiant at 32° +51°, for July 25–31, mentioned in these "Notes" for last month; and there can be no doubt that its meteors have hitherto been mistaken for the true *Perseids*, and thus given this well-known shower an undue extension of period on the preceding side. The end of July was formerly pointed out by Quetelet as a meteor-epoch of considerable intensity, and these rich displays of *Aquarids* and *Perseids* II. fully confirm the opinion. After Aug. 2 an interval of 4 cloudy nights followed; and when observations were resumed on the 7th and 8th (44 meteors), the new shower of Perseids had become exhausted and given way to the ordinary *Perseids* near η Persei. Of the minor radiants observed this year on July 26–Aug. 2, the following were the most exactly determined:—12° +70° (16 meteors), 332° +50° (14), 6° +37° (18), $\left.\begin{array}{l}11^\circ +47^\circ \\ 12^\circ +52^\circ\end{array}\right\}$ (26), 28° +36° (12), 333° +9° (10), 291° +70° (14), 321° +31° (10), 332° +37° (11), 332° +27° (8), 333° +18° (8), 7° +11° (8), 3° +27° (8), 23° +41° (8). Several very fine meteors = ♀ and ♃ were recorded. On July 21 two were seen from a radiant at 234° +48°. On July 30, at $13^h\ 44^m$, a meteor was seen falling between δ Capricorni and Fomalhaut with a very swift motion and brightening at the end in a flash equal to Venus. It had a short path of about 6°, and left a streak there about 3° long for 4 seconds. This meteor was directed straight from Tupman's radiant (No. 44) at 326° −13°, July 28. Three meteors = ♃ were also seen from the shower at 32° +53°. Moonlight interfered with observations of the *Perseids* on Aug. 10; but there were no clouds, and the sky was watched between $10^h\ 30^m$ and 15^h. About 130 meteors were seen during that time. Between $14^h\ 30^m$ and 15^h, when the moon had set, 33 were counted. Last year, the half-hourly number between $12^h\ 30^m$ and $14^h\ 30^m$ on Aug. 10 was 42; so that the display of this year appears to have been less important than in 1877. The directions of the foreshortened paths near the radiant were very carefully recorded and found to converge on *two* centres, each well supported, namely:—

I. At 44° +59° near B Camelopardi.
II. At 42½° +54° near γ–η Persei.

The former (I.) agrees with the radiant as previously observed by the writer on Aug. 10 in 1874, 1876 and 1877, and accords with the Perseid Comet (III. 1862) with radiant at 43° +57½°, Aug. 10. The latter (II.) agrees with Heis's A_{11} at 45° +52°, from which he traced a vast number of meteors in Aug., and falls near Comet I. 1870, with radiant at 43½° +53°, Aug. 12 (see Prof. Herschel's "List of the radiant-points of Comets," p. 232, B. A. Report, 1875). The rich and sustained shower of *Perseids* in 1871 may thus have been attributable in part to Comet I. 1870. In order further to investigate the point, the writer projected about 140 of the meteors near the Perseid radiants registered by the Italian observers on Aug. 9–12, 1872, and found three centres indicated, namely, at 47° +58°, 42° +52°, 33° +57°. While the two former correspond to the double radiant seen this year, the latter agrees with a radiant deduced by Prof. Herschel from the meteors recorded by the British Association observers on Aug. 9, 10, 11 in 1871. The three positions are each confirmed by stationary meteors. On Aug. 5, 1864, Heis records one = ♀ at 44° +57°, and on Aug. 9, 1850, at 43° +53°, and at Bristol, on Aug. 8, 1876, one was seen at 35° +57°. In future observations of the *Perseids* it will be very important to observe the short and nearly stationary meteors, so that these radiants may be confirmed and separated from each other. The recent observations go far to prove that the alleged early commencement of the shower is explained by the occurrence of a distinct system of similar meteors on July 30–Aug. 1 from a point near χ Persei, and that the diffuse radiation hitherto observed on Aug. 10 is entirely due to the fact of several contemporary showers lying near together about B Camelopardi, γ Persei, and χ Persei, supplying meteors of the same visible features in common.

W. F. Denning.

Sept.	G. M. T.	PHENOMENA.
	h m	
15. S.	10 49	26 Ariet. Oc. †333°.
16. M.	8 15·9	♃ tr. D. 21° 23′ S.
	12 16·8	♈ transits.
	12 20·5	♄ tr. D. 2 22 S.
	12 23	♃ sets.
	14 47·1	♆ tr. D. 12 56 N.
	15 42	♀ rises.
	17 40	☉ rises.
	22 41·7	♀ tr. D. 11 2 N.
	23 6·7	☿ tr. D. 5 52 N.
17 Tu.	23 54·4	☉ tr. D. 2 13 N.
	6 9	☉ sets.
	8 10	Twilight ends.
	9 8	♃ 1 Sat. Oc. D.
	15 39	χ′ Tauri Oc.† 167°
18. W.	0	♂ ☌ ☉
	7	☿ stationary.
	7 24	♃ 1 Sat. Sh. I.
	8 36	♃ 1 Sat. Tr. E.
	9 34·8	♃ 4 Sat. Ec. R.
	9 44	♃ 1 Sat. Sh. E.
19. Th.	7 2·8	♃ 1 Sat. Ec. R.
☾	6 30·0	
20. F.	8 58	♃ 2 Sat. Oc. D.
	10 43	ω Gem. Oc.† 183°.
	15 7	48 Gem. Oc. R. 220.
21. S.	7 15	♃ 3 Sat. Sh. I.
	22 45·5	♀ tr. D. 8° 50′ N.
	22 50·6	☿ tr. D. 7 9 N.
22. S.	4	♄ ☍ ☉
	7 2	♃ 2 Sat. Tr. E.
	9 24	♃ 2 Sat. Sh. E.
23. M.	2	♃ stationary.
25. W.	8 7	♃ 1 Sat. Tr. I.
	9 19	♃ 1 Sat. Sh. I.
	10 27	♃ 1 Sat. Tr. E.
	14	☿ elong. 17° 50′ W.
	17	☿ Perihelion.
26. Th.	7	☾ Perigee.
●	2 10·3	
	8 25	♃ 4 Sat. Tr. E.
	8 58·1	♃ 1 Sat. Ec. R.
	7 36·3	♃ tr. D. 21° 23′ S.
	11 38·4	♄ tr. D. 2 41 S.
	11 37·5	♈ transits.
	22 47·8	☿ tr. D. 6 32 N.
	22 49·0	♀ tr. D. 6 31 N.
27. F.	23 51·0	☉ tr. D. 1 41 S.
28. S.	9 52	♃ 3 Sat. Tr. E.
29. S.	6 34	♃ 2 Sat. Tr. I.
	9 5	♃ 2 Sat. Sh. I.
	9 30	♃ 2 Sat. Tr. E.
Oct.		
1. Tu.	22 52·3	♀ tr. D. 4° 8′ N.
	22 54·1	☿ tr. D. 4 21 N.
2. W.	9 59	♃ 1 Sat. Tr. I.
☽	19 0·9	
3. Th.	7 19	♃ 1 Sat. Oc. D.
4. F.	6 47	♃ 1 Sat. Tr. E.
	8 4	♃ 1 Sat. Sh. E.
	10 1	4 Capr. Oc.† 153°
5. S.	8 54	θ Capr. Oc. D. 39°
	10 7	θ Capr. Oc. R. 258°
6. S.	9 5	♃ 2 Sat. Tr. I.
	6 58·1	♃ tr. D. 21° 20′ S.
	10 56·3	♄ tr. D. 2 59 S.
	10 58·1	♈ transits.
	11 6	♃ sets.
	13 26·9	♆ tr. D. 12 48 N.
	16 43	♀ rises.
	18 12	☉ rises.
	22 55·5	♀ tr. D. 1 41 N.
	23 4·7	☿ tr. D. 1 10 N.
7. M.	23 47·8	☉ tr. D. 5 34 S.
	5 23	☉ sets.
	7 20	Twilight ends.
8. Tu.	8 45·0	♃ 2 Sat. Ec. R.
9. W.	8 52·5	♃ 3 Sat. Ec. R.
10. Th.	4	☾ Apogee.
	9 12	♃ 1 Sat. Oc. D.
○	20 54·4	
11. F.	6 20	♃ 1 Sat. Tr. I.
	7 39	♃ 1 Sat. Sh. I.
	8 40	♃ 1 Sat. Tr. E.
	14 23	101 Pisc. Oc. R. 223
	22 58·6	♀ tr. D. 0° 47′ S.
	23 16·5	☿ tr. D. 2 27 S.
12. S.	7 17·7	♃ 1 Sat. Ec. R.
	19 6	26 Ariet. Oc. R. 300°
13. S.	5 53	ε Ariet. Oc. D. 67°
	6 46	ε Ariet. Oc. R. 252°
	9 16	♃ 4 Sat. Sh. I.
14. M.	6 19	27 Tauri Oc. R. 253°
	6 21	28 Tauri Oc. R. 272°

† A near approach.

EDITOR.

Distances from Saturn's polar diam., in equatoreal semidiams. — means preceding or west, + following or east.

12h G.M.T.	Encel.	Tethys.	Dione.	Rhea.
Sept. 16	−3·4 S	+4·2 S	−1·7 N	+4·2 S
17	−1·8 N	−4·6 N	−3·5 S	+8·5 N
18	+3·8 N	+4·9 S	+6·3 N	−1·1 N
19	+0·8 S	−5·0 N	−4·9 N	−8·9 N
20	−4·0 S	+4·9 N	+0·2 S	−2·0 S
21	+0·2 N	−4·7 S	+4·6 N	+8·2 S
22	+4·0 S	+4·3 N	−6·4 N	+4·9 N
23	−1·2 S	−3·7 S	+3·8 S	−6·4 N
24	−3·7 N	+3·0 N	+1·3 N	−7·2 S
25	+2·1 N	−2·3 S	−5·5 S	+3·9 S
26	+3·1 S	+4·8 N	+6·1 S	+8·6 N
27	−2·9 S	−0·5 S	−2·5 N	−0·8 N
28	−2·4 N	−0·5 N	−2·7 S	−8·9 N
29	+3·5 N	+1·4 S	+6·1 N	−2·4 S
30	+1·5 S	−2·3 N	−5·4 N	+8·1 S
Oct. 1	−3·9 S	+3·1 S	+1·1 S	+5·2 N
2	−0·6 N	−3·7 N	+4·0 N	−6·2 N
3	+4·0 S	+4·3 S	−6·4 S	−7·4 S
4	−0·4 S	−4·7 N	+4·5 S	+3·5 S
5	−3·9 N	+4·9 S	+0·5 N	+8·7 N
6	+1·4 N	−5·0 S	−5·1 S	−0·5 N
7	+3·6 S	+4·9 N	+6·3 S	−8·9 N
8	−2·3 S	−4·6 S	−3·3 N	−2·7 S
9	−3·0 N	+4·1 N	−1·9 S	+7·9 S
10	+3·1 N	−3·6 S	+5·8 N	+5·5 N
11	+2·2 S	+2·8 N	−5·8 N	−5·9 N
12	−3·9 S	−2·0 S	+1·9 S	−7·6 S
13	−0·6 N	+4·9 N	+3·3 N	+3·2 S
14	+4·0 N	−0·2 S	−6·3 S	+8·8 N
15	+0·3 S	−0·7 N	+5·0 S	−0·1 N

Semiaxes of orbits expressed in semidiameters of Saturn's equator.

Oct. 2.	Mi.	En.	Te.	Di.	Rh.	Ti.	Iap.
Major	3·14	4·03	4·99	6·40	8·93	20·71	60·34
Minor	0·15	0·20	0·24	0·31	0·44	0·94	8·72
Hourly Motion	15°·9	11°·0	8°·0	5°·5	3°·3	0°·9	0°·2

The position-angles of the axes of the orbit of Titan are 0°·56, and those of the orbit of Iapetus 12°·36 *less* than those of the axes of the orbits of the inner satellites.

Iapetus is near its greatest eastern elongation on Sept. 21, and near its inferior conjunction, north of the planet, on Oct. 11.

A. M.

Satellite of Neptune.

Semi-major and minor axes of apparent orbit on Oct. 2, 16″·9 and 5″·9, pos.-angle of major axis being 221°·4 or 41°·4. The satellite is near its greatest elongation at the following times:—

Sept. 17 16h *n.f.*	Sept. 26 11h *s.p.*	Oct. 5 7h *n.f.*	Oct. 14 3h *s.p.*
20 14 *s.p.*	29 10 *n.f.*	8 6 *s.p.*	17 1 *n.f.*
23 13 *n.f.*	Oct. 2 9 *s.p.*	11 4 *n.f.*	20 0 *s.p.*

A. M.

Position of the Sun's North Pole.

1878.	Pos.-angle. ° ′	Dist. from limb. ° ′
Sept. 17	24 41	+7 8
22	25 23	6 59
27	25 55	6 48
Oct. 2	26 17	6 34
7	26 29	6 16
12	26 29	+5 56

The Moon.

1878.	Rises. h m	Geoc. Libr. at midnight. long. °	lat. °	1878.	Sets. h m	Geoc. Libr. at midnight. long. °	lat. °
Sept. 16...	7 18	−5·02	−6·58	Oct. 1...	7 52	+7·84	+4·97
17...	7 47	5·99	6·18	2...	8 55	8·00	3·82
18...	8 27	6·75	5·49	3...	10 9	7·72	2·51
19...	9 21	7·23	4·51	4...	11 24	7·08	+1·11
20...	10 29	7·37	3·28	5...	12 39	6·15	−0·31
21...	11 50	7·10	1·83	6...	13 52	5·03	1·69
22...	13 15	6·40	−0·22	7...	15 2	3·79	2·98
23...	14 44	5·23	+1·44	8...	16 11	2·48	4·13
24...	16 13	3·64	3·06	9...		+1·15	5·10
25...		−1·71	4·50	10...	Full.	−0·16	5·84
26...	New.	+0·40	5·63	11...		1·43	6·33
27...		2·52	6·36	12...	Rises.	2·63	6·55
28...	Sets.	4·46	6·63	13...	5 24	3·75	6·48
29...	6 25	6·06	6·46	14...	5 51	4·78	6·11
30...	7 2	+7·19	+5·88	15...	6 27	−5·67	−5·46

A. M.

The Planets.

Mercury is a morning star at greatest elongation W. on Sept. 25, when he rises $1^h 45^m$ before the Sun, and, though then in perihelion, is very favourably placed for observation on account of his N. declination. He is very near Venus about the end of September, being only 17′ from her on Sept. 26 ($1^m 10^s$ preceding and 1′ N.).

Venus is a morning star, rising from 2^h to $1\frac{1}{4}^h$ before the Sun. Her disk is gibbous, the illuminated portion on Sept. 15 being 0·936, and on October 15 0·976, and the corresponding diameters 10″·6 and 10″·0.

Jupiter is an evening star, stationary about 7° *s. p.* β Capricorni. His equatoreal diameter is 41″·6 on Sept. 15 and 37″·9 on Oct. 15.

Saturn is visible all night, about midway between β Ceti and α Pegasi, moving slowly *s. p.*

	Outer Ring.		Inner Ring.		Ball.
	Maj. Axis.	Min. Axis.	Maj. Axis.	Min. Axis.	Diam.
Sept. 22	43″·95	2″·44	29″·22	1″·62	17″·6
Oct. 12	43″·64	1″·89	29″·02	1″·26	17″·4

The south side is visible, the elevation of the Earth above the plane of the rings being 3° 11′ S. and 2° 29′ S. on Sept. 22 and Oct. 12 respectively, and of the Sun 3° 24′ and 3° 42′ S.

EDITOR.

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Binary Stars.—Part III.

THE observations on binary stars embrace so short an interval of time that we but seldom have been able to decide any thing about the orbit where the period is above three hundred years, and even then it is necessary that the periastron-passage happen no more than a hundred years from now, if the eccentricity is not unusually small. However, I think we have already now a pretty close grasp of the orbit of σ Coronæ Borealis, though the period is above eight hundred years, while the orbits of α Geminorum and γ Leonis are not known with certainty. At most we possess orbits of twenty-seven binary stars, of which only seven belong to the southern hemisphere; but a number of quickly revolving binaries will be found in future, and Burnham's discoveries seem fertile in this respect.

The peculiar systematic errors which are known to affect the measures of double stars render exhaustive discussion of the material previous to use an important condition *, and it is evident that the accuracy of the results will be greatly improved as one gains experience on the subject; but I nevertheless hasten to comply with a request from the editor of 'The Observatory' and give a review of my researches, though it should be remembered that I have been engaged in this work during only between three and four years, and that I expect greatly to extend my investigations in years to come.

In the following elements of orbits of binary stars

☊ denotes the longitude of the node.
λ the longitude of periastre, reckoned from the node.
γ the inclination of the orbit to the visual plane.
e the eccentricity of the true orbit.
P the period of revolution in years.
T the epoch of periastron passage.
a the semi-axis major of the true orbit.

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* Double stars are now-a-days observed in over twenty large observatories. Small series of measures taken by inexpert amateurs would therefore seem to be of minor importance, so much more as want of system prevents thorough examination of a small number of results. In some such cases it might even be difficult to ascertain whether the measures were made *bonâ fide*.

Σ 3121.

The anonymous binary star Σ 3121 was discovered by W. Struve in 1831. It has a large proper motion. Both components are white or perhaps a little yellowish, and so like each other that the companion has often been mistaken for the main star. They are of the seventh magnitude. The right ascension of the star is about 9^h; but the circumstance that it was placed in an appendix caused most observers to lose sight of it, except O. Struve, who has observed it from 1840 up to this time. In the spring of 1866 his assistant Fritsche computed the orbit by Herschel's second method, following up with differential equations of condition. This investigation being necessarily founded on the measures made by both Struves, as there were no others available, afforded an opportunity for testing the very large systematic corrections which O. Struve had applied to his observations. The result showed that the probable errors came out smaller when these corrections were applied than when they were not, but the elements soon showed considerable deviations from the observations. According to them we should not have seen the star single before 1882, while the companion was already lost sight of in 1876, and the ephemeris deviated at that time half a hundred degrees from the observed angle. By Fritsche's paper attention was drawn to the interesting object. It has since been well watched; but the results of the other observers differed so considerably from O. Struve's that I had to exclude them all, preferring to adopt Struve's values after 1866, as had to be done before that epoch. I formed twelve normal angles, and applied Klinkerfues's second method to them; but Fritsche's elements were not correct enough to allow the orbit to be derived in this manner. I only obtained a correction to the period, and after applying this I obtained corrections to the three elements e, P, and T from six of the places. From three of the angles in connexion with the true anomalies computed from the new elements I obtained ☊, λ, and γ, which were repeatedly corrected by equations of condition by the method of least squares. With the last result, together with the eccentricity, I now calculated the mean anomalies from the six observed angles, and from these mean anomalies I obtained P and T by the method of least squares. Next I compared the observed angles with the ephemeris. The differences $\theta_o - \theta_c$ were projected as ordinates with the time as abscissa, and a curve drawn as well as I could manage among the points. On this curve I read off the corrections to be applied to the ephemeris, and constructed then a curve on which the angles are read off as ordinates with the epochs as abscissæ. I determined the distances from this curve; but a curve drawn through so small a number of points is subject to much uncertainty. I therefore had to consider what the curvature would have been if the curve had been drawn in a somewhat different manner, and thereby I estimated whether the respective distances would have to be lessened or increased. I then projected the angles and distances as in Herschel's first method, and indicated with arrows whether the distances might be lessened or increased. Thus I obtained two different systems of elements, after a number of interpolations between which I arrived at the following result. Though this is by no means a definitive orbit, still I think it about as accurate as any that might be deduced before the companion appears again.

Provisional Elements of Σ 3121.

	°	′		
☊	16	0	e	0·2600
λ	149	30	P	37·03 years
γ	74	15	T	1842·78
			a	[0″·71]

γ *Coronæ Borealis.*

This is one of the most difficult objects discovered by W. Struve. It is of the fourth magnitude, and, as Dawes has shown, not variable. The orbit is seen nearly edgewise, so that the star occasionally appears single. Thus the companion was lost even in the best telescopes in 1872, and has not yet been seen again with certainty. I commenced my investigations by forming three complete normal places. From these and a fourth place for 1867 (for which the angle was obtained from Harvard College, the distance from Leipzig observations), the first approximation to the elements was obtained by Herschel's method. As four points are not sufficient to determine an ellipse, I took the other observations into account by estimation in drawing the apparent orbit, and afterwards I endeavoured to represent the three normal places and the angle for 1867 exactly, by Klinkerfues's third method. The calculations showed, however, that these observations could not be represented by any ellipse; but just as I was going to compute a hyperbolical orbit I was informed that the angle given for 1867 was nearly a whole quadrant in error. Subsequently I came into possession of O. Struve's observations, and made an attempt to compute the orbit by Herschel's second method, but failed. The companion moved almost in a straight line up to 1868, and afterwards the star was difficult to observe, and the recorded distances probably too large, so that the case was most unfavourable for application of this method; but I succeeded in obtaining a couple of orbits graphically, and I corrected these by equations of condition, whereby I determined the relative weight to be attributed to the respective position-angles by successive trials.

Elements of γ Coronæ Borealis = Σ 1967.

	°	′		
☊	110	24	e	0·350
λ	233	30	P	95·50 years
γ	85	12	T	1843·70
			a	0″·70

ξ Libræ.

The components of ξ Libræ are both between the fourth and the fifth magnitude, and pale yellow. It was discovered by W. Herschel in 1782. The smaller and further companion is blue or violet. This star is ξ Scorpii of Bayer, and was only by a mistake called 51 (ξ) Libræ by Flamsteed. Those astronomers, however, who have treated about it as a binary star have called it as above, and I prefer it on that account. Further discussion about such a mere denomination would seem idle. The apparent ellipse which I constructed had its centre at the principal star, and the true orbit appeared thus to be circular. The circular elements were repeatedly corrected by the method of least squares; and I then assumed $e=0{\cdot}1$, in order to find out any feeble eccentricity there might be in it, and λ successively 90° and 180°.

Elements of ξ Libræ = Σ 1998.

	°	′		
☊ ……	12	15	e ……	0·0768
λ ……	89	16	P ……	95·90 years
γ ……	68	42	T ……	1859·62
			a ……	1″·26

Σ 3062.

The interesting binary Σ 3062 was discovered by Herschel in 1782. Both the larger and the smaller star are about the seventh magnitude, and both white. The determination of the orbit does not offer any difficulties. I constructed it principally on W. Struve's and Dembowski's measures. Afterwards the five elements were corrected by equations of condition. The corrections came out small, but failed to reduce the errors in spite of the evident certainty of the drawing. But on adding the term depending upon the eccentricity the corrections came out larger, and the errors were exceedingly small. In this case the cause of the delay was therefore simply that the eccentricity had not been obtained with sufficient accuracy by drawing.

Elements of Σ 3062.

	°	′		
☊ ……	38	35	e ……	0·4612
λ ……	92	7	P ……	104·415 years
γ ……	32	11	T ……	1834·88
			a ……	1″·27

ω Leonis.

ω Leonis was discovered by Herschel in 1782. The components are white, and of the sixth and seventh magnitude respectively. The last orbit had been determined by Klinkerfues from a very small number of observations by Herschel's first method, applying subsequently insignificant corrections, according to his second method. The differences of the observed angles from an ephemeris computed from this orbit were projected as ordinates on the square paper, with the time as abscissa and the deviation curve drawn, after which the interpolating curve for the angles was constructed and at the same time the observed angles projected. Both curves were very interesting, and their remarkable points were discussed and compared with those corresponding to other orbits. Much insight into the nature of the orbit is to be gained in this way. As the elements were not very satisfactory, the interpolating curve was repeatedly altered, and the corresponding changes in the elements ascertained, P and T being the whole time computed from position-angles corresponding to 1782 and 1872. By successive interpolations at last an approximate orbit was obtained, which was corrected by a number of systems of equations of condition. It deserves to be remarked that, although O. Struve's observations were exclusively employed since 1840, still the orbit occasionally took an intermediate course between the results obtained by this observer and those obtained by other observers. My latest elements are as follows:—

Elements of ω Leonis=Σ 1356.

	° ′		
☊	147 15	*e*	0·5510
λ	122 19	P	114·55 years
γ	64 5	T	1841·57
		a	0″·85

p Eridani.

This double star was discovered by Dunlop in 1825. The components are of the sixth magnitude, and both grey. I obtained the orbit by correcting Jacob's elements. A weight of one fourth was allowed to Dunlop's result throughout. He appears to have been a very untrustworthy observer.

Elements of *p* Eridani.

	°		
☊	81 42	*e*	0·378
λ	327 15	P	117·51 years
γ	44 40	T	1817·51
		a	3″·82

Σ 1768.

W. Struve discovered Σ 1768 in 1829, when the components, which are of the sixth and eighth magnitude respectively, and both white, were a second apart. The distance decreased rapidly, and the star appeared single in 1859. It was reobserved in 1876 by Schiaparelli, the reappearance having been suspected several years before by some other observers. The elements were graphically obtained, furnishing an excellent illustration of the adaptation of Herschel's first method even to the most difficult cases. The particulars of the operations were very like those by which the orbit of 44 Boötis was determined. I am calculating the definitive orbit.

Provisional Elements of Σ 1768.

☊ …… 82°	e …… 0·66
λ ……202	P…… 124·5 years
γ …… 51½	T……1863·0

ξ *Boötis.*

Herschel discovered ξ Boötis in 1780, and observed it afterwards repeatedly. The components are of the fourth and seventh magnitudes respectively. Their colours present a beautiful contrast, the larger being orange, the smaller purple. The first observed distances were too large, a circumstance which hitherto much impeded the derivation of the elements. The definitive orbit given below did not represent the distances quite so well as the one I first determined; and this shows that the first measures of this coordinate were not only affected with large accidental errors, but also with those deviations subsequently known as systematic errors. I altered the angle in 1793 according to "Nature," and drew interpolating curves both for the angles aud the distances, correcting one curve after the other. The elements were then obtained from Herschel's first method, and they were subsequently corrected by different systems of equations of condition, for which I estimated a slight difference in weight from trials.

Elements of ξ Boötis = Σ 1888.

☊ …… 26° 22′	e …… 0·7081
λ ……117 46	P…… 127·35
γ …… 36 55	T……1770·69 years
	a …… 4″·86

W. Doberck.

[To be continued.]

The Pulkowa Double-star Observations.—Part I.

One of the most interesting and important works in the department of sidereal astronomy that has appeared during the present year is undoubtedly Volume ix. of the 'Observations de Poulkova,' recently published by M. Otto Struve, the able and energetic Director of the Imperial Observatory at Pulkowa. This volume contains the micrometrical measures of double stars made at Pulkowa from 1839 to 1875; and as, in addition to the observations themselves, the Introduction contains some curious and important researches on the subject of double-star observations, I have, at the suggestion of the Astronomer Royal, drawn up an account of the book, which will perhaps interest the readers of 'The Observatory.'

Since the foundation of the Imperial Observatory in 1839 the great telescope has been principally employed to study the relative motions of stellar systems, and though occasionally interrupted by other researches, the work has been persevered in up to the present time. Thus a long series of observations has been obtained which, being made by one person and always in the same manner, has the advantage of furnishing data of strictly the same character throughout, and in which the personal errors have only changed with the advancing years and more mature experience of the observer. The task which M. Struve has set himself, and for the accomplishment of which he has laboured for so many years, is to furnish data as exact as possible for the study of the laws which regulate the relative motions in double and multiple systems of stars. The progress of science and the extension of our knowledge concerning binary systems have from time to time altered the character and direction of the work; and this is also the cause of the long delay in publication, as M. Struve was unwilling to publish his observations till he had accomplished his task, at all events within certain limits.

The first working list drawn up by M. Struve contained about 450 double and multiple stars; these consisted, in the first place, of double stars, for which the 'Mensuræ Micrometricæ' of W. Struve had already indicated a relative motion, or for which further observations were necessary to confirm suspected movements. This list was afterwards extended, so as to contain all W. Struve's double stars for which the distance of the components is less than 1″, and the brighter pairs (*lucidæ*) for which the distance varies from 1″ to 2″. Finally M. Struve augmented his list by the introduction of several double stars of each order of distance, which, according to former measures, were not subject to rapid changes of relative position, and which, for this reason, would serve as points of comparison for distances measured by him and by his father; there were also added 135 stars of Herschel's Classes V. and VI. By means of observations of these objects M. Struve hoped to arrive at very exact conclusions as to the relative frequency of binary systems in the different orders of distance. The stars in this preliminary working list extended down to —15° Dec., but experience soon showed that very little reliance could be placed on observations of objects situated to the south of the Equator; M. Struve accordingly resolved to confine himself to stars in the northern hemisphere, except in the case of some very interesting systems, the R.A. of which would allow of their being observed in the early evening hours in spring or summer, when the state of the atmosphere is most favourable for astronomical observation.

M. Otto Struve succeeded his father as Director of the Observatory in 1858, and, of course, the duties of his position interfered, to a great extent, with the double-star observations; still they were persevered in with as great regularity as was possible under the circumstances. The list of objects to be observed was also still further increased by the introduction of all the double stars in the northern hemisphere discovered by Dawes, Alvan Clark, Burnham, and others, when, by reason of their brightness and distance of the components, they fall within the limits adopted for the Pulkowa observations. Lastly there was added to the list a number of double stars chosen to serve as objects of comparison between M. Struve's measures and those of other astronomers; these observations were undertaken, in the first instance, in concert with Baron Dembowski, whose long series of excellent micrometer measures gain hereby additional importance and utility, since by these measures they can be combined with the observations of M. O. Struve, and by this means with those of W. Struve. The observations of all these stars fill two volumes; the first (volume ix.) contains 4030 measures of 478 stars of the Dorpat Catalogue, and 2050 measures of 427 stars discovered at Pulkowa. The second volume (volume x.) will contain the measures of double stars the distances of the components of which exceed 32″, the determinations of relative parallax, and the observations of double stars discovered by other astronomers.

The telescope and micrometer used by M. Struve in these researches have been fully described in the 'Description de l'Observatoire Central par F. G. W. Struve' (1845); the following is probably a sufficient explanation for the present purpose. The telescope is a refractor of the form known as the "Fraunhofer" equatoreal, the aperture of the object-glass of which is 14·93 inches and the focal length 22·55 feet. It was constructed by Merz and Mahler at the celebrated establishment of Utzschneider and Fraunhofer in Munich, and at the time of its construction was one of the great instruments of the world both in respect to size and also on account of its excellent optical qualities. And though it has been in constant use for 37 years and exposed to the extraordinarily rapid changes of temperature experienced in the Russian climate, it seems to have deteriorated very little, except perhaps in the parts constructed of metal. It appears from a series of experiments made by M. Struve that the focal length of the telescope alters 0·0101 in. for a change of temperature of one degree Réaumur—a quantity sufficient to sensibly affect the images. On this account it has been M. Struve's practice to correct the focus several times each evening, especially when the changes of temperature are great. These corrections would be of much smaller amount if the tube of the telescope, instead of being composed of wood, were of metal.

The excellent quality of the images formed by the telescope (which is fully demonstrated by the observations themselves) is, no doubt, due in great measure to the very suitable locality in which it is placed. The position of the Observatory, on a hill 160 feet above the level of the surrounding plain, and its considerable distance from thickly inhabited districts, secures a clearness of the atmosphere such as is very seldom found in other observatories, and also secures freedom from the undulations caused by currents of heated air which are so troublesome when chimneys are in the immediate neighbourhood.

For double-star observations the construction of the micrometer is of the most vital importance, as, if this is faulty, the measures are vitiated to a corresponding extent. M. Struve has taken care to describe his apparatus in considerable detail, so as to enable his readers to judge for themselves what amount of reliance can be placed on his measures. The telescope is provided with two filar micrometers, as identical in construction and dimensions as the artists (Merz and Mahler) could produce; M. Struve has almost always made use of one of these micrometers, the second being only employed whilst the first was undergoing repair. The eyepieces, nine in number, are equally adapted to both micrometers, and their magnifying-powers range from 138 to 1822. In the observations of double stars, properly so called, viz. when the distance is less than 32″, M. Struve has used almost exclusively powers from 413 to 1169; for objects of 32″ to 4′ distance powers 207 and 309 have generally been used; occasionally for very close pairs the power 1458 has been employed, when the atmospheric conditions were peculiarly favourable, but it has never been possible to use the highest power with advantage. The position-circle of the micrometer is 8 inches in diameter, and can be easily read off to minutes by means of two opposite verniers. The micrometer apparatus is applied to the telescope so firmly that any gradual change of zero of the position-circle, as defined by the direction of the diurnal motion, has never been satisfactorily established; although, of course, the necessity for taking the micrometer off from time to time for purposes of cleaning &c. has caused a slight change in the zero. The micrometer is provided with a single pair of parallel wires, with small adjusting screws, so that the parallelism may be always maintained, and the thickness of the wires varies from time to time. Ordinarily M. Struve has used spider-lines as fine as it was possible to procure, the diameter being about $0''\cdot15$; but the thickness, of course, increased after a time by the adhesion of particles of dust to the threads, so that the mean diameter might perhaps be taken to be about $0''\cdot2$ or $0''\cdot3$. The two threads are fixed on two frames moving independently in their grooves by means of two fine screws. One of these screws, the micrometer-screw proper, is used for actually measuring distances; the other screw, moving the frame which carries the so-called fixed thread, allows the observer to change the points of coincidence, the zero of the measures, along the micrometer-screw. The fixed thread is approximately in the optical axis of the telescope when the coincidence of the threads occurs at 56 rev. of the micrometer-head. All the measures have been taken at about this reading, the point of coincidence always falling between 46 rev. and 66 rev.

The angular value of a complete revolution of the screw $=9''\cdot7091 - 0''\cdot00022\,t$, where t is the temperature expressed in degrees Réaumur. The coefficient of t is deduced from the changes produced by temperature on the focal length of the telescope, of which the result is given above. The value of a revolution for $t=0$ was determined by two methods, which M. Struve considers to be free from every objection—first, by measurements of terrestrial objects the distances of which from the telescope could be rigorously determined; and, secondly, by the measurements of a series of differences of declination between two stars of which the sum, being equal to the difference of declination of the extreme stars, could be exactly determined by the meridian instruments. By the first method M. Struve finds

$$r = 9''\cdot7083 \pm 0''\cdot0017,$$

and by the second

$$r = 9''\cdot7117 \pm 0''\cdot0031,$$

and by combining these according to their weight he obtains the definitive value given above. Having thus determined the mean angular value of a revolution of the micrometer-screw, it was necessary in the next place to examine the screw itself to determine the differences between the values of successive revolutions, to find the periodic errors, and to ascertain by direct comparison if the two micrometers were actually identical. It is unnecessary to enter into the details of these operations here; suffice it to say that both species of errors are, according to M. Struve, very small and quite insensible for measurements of close stars, and that the two micrometers are practically identical when employed to measure small distances.

For my own part I must say that I think it is a pity that M. Struve has made no use of the double-image principle. The double-image micrometer, as devised by Sir G. B. Airy and so extensively used by the late Prof. Kaiser of Leiden, would have been, one would have thought, the very thing for these measures. I very much doubt if any measure with a wire micrometer is comparable in accuracy to those made with a double-image instrument; and the latter is, I should think, much more free from the troublesome personal equations against which M. Struve has fought so fiercely.

We now come to the observations of artificial double stars, which demand a more detailed account than the previous sections of this very interesting Introduction.

In 1852, M. Struve commenced his researches on the subject of personal errors, adopting the same method as that previously used by his father at Dorpat. A board two feet long and nine inches wide, with blackened surface, was set up, at right angles to the direction of the telescope, on the upper gallery of an old tower used for telegraphic purposes, situated near the village of Pulkowa, at the distance of about two versts and a half to the east of the Observatory. On the black face of the board 24 white circles were placed, so as to form 12 pairs of artificial stars, the distances between the centres of which, as seen through the telescope, subtended angles of from 1″ to 2″. Down the entire length of the board a white line was traced to serve as the normal direction to which the measured position-angles of the different pairs might be referred. This first attempt led to suprising results, proving that even for objects 2″ apart the differences between the measured and actual directions sometimes amounted to as much as 10°, without giving any distinct law for these differences. This circumstance gave rise to the suspicion that perhaps the measures were influenced by the simultaneous presence, in the field of the telescope, of a great number of stars, and particularly of the white line, which made it difficult to estimate when the micrometer-wires were parallel to the directions to be measured. M. Struve accordingly resolved to attack the problem anew the following year, avoiding the difficulties mentioned above, and increasing the number of directions to be measured to such an extent that the law of the errors, if there were any law, could not escape detection. With this end in view, he constructed the apparatus which was used in all subsequent researches, and which appears to have answered his purpose sufficiently well. This consisted of a circular iron disk 35 inches in diameter, and 0·2 inch thick, which was perforated at the centre and carried, at this point, a hollow axis, forming exactly a right angle with the plane of the disk. By means of a very strong plate fixed to this axis the whole apparatus was mounted on the gallery of the telegraph tower. Before the commencement of each series of measures, for the purpose of ascertaining that the plane of the disk was perpendicular to the line of sight, the position of the axis was verified by directing it, by means of lenses inserted in it, as nearly as possible on the object-glass of the telescope, the axis being provided with small adjusting screws so that its position could be slightly altered. This being done the aperture where the axis penetrated through the centre of the disk was closed by a black button. The artificial stars were formed by the bases of small ivory cylinders carefully worked to a circular figure, and placed in corresponding holes in the disk which were made for their reception. The white bases of these cylinders contrasting with the dull black colour of the disk, M. Struve states to be wonderfully like real stars seen in favourable conditions of the atmosphere, their apparent diameters, in the telescope, ranging from 0″·66 to 0″·20. Ordinarily only two stars were viewed at one time; and whilst these were being measured, the other cylinders were replaced by black buttons. Two of these artificial stars, situated near the circumference of the disk at opposite ends of a diameter, were used as points of reference from which to deduce the true position-angles of close pairs from comparison with the measured directions. For this purpose these two stars had to be visible at the same time as the stars to be examined; but on account of their isolated position at the circumference of the disk, their presence in the field of the telescope never interfered with the measures of other objects. The angular distance between these reference stars, amounting to nearly a minute of arc, always allowed of their true direction being determined with ease to a small fraction of a degree. It is then only necessary to determine, by direct measures made on the disk itself, the true angle formed by the intersection of the line joining any pair of stars with the direction of the two reference stars in order to obtain the true direction which is required for comparison with the observed position-angle.

The first series of observations for the determination of personal errors was made during the years 1853, 1855, and 1856, the second in 1866, and the last in 1876. During the interval between the first two series the old tower on which the apparatus was placed was pulled down; in its place M. Struve made use of a wooden pyramid, erected at a short distance from the site of the tower, for geodetical purposes. Before the commencement of the operations in 1876, this pyramid was replaced by another, as there was reason to fear that the rotten beams of the first could no longer support the weight of the apparatus and of the assistant who had to be stationed there during the observations. The linear distance between the disk and the object-glass of the telescope has been very carefully determined for these three positions, and the result is

for 1857, dist.=8888·3 feet.
„ 1866, dist.=8852·1 „
„ 1876, dist.=8848·9 „

M. Struve is confident that he has determined the distance with great accuracy, and states that the probable error of the last determination =6·5 inches.

All the measures of the artificial stars have been made in summer during the afternoon hours, when the images of terrestrial objects are steadiest; and it has generally been found that a clear sky is most favourable for these observations. The choice of the objects to observe, and the magnifying-powers to use has depended chiefly on the state of the images; during the operations in 1866 and 1876 M. Struve has usually employed for the measures of artificial stars the same magnifying-powers as he has been accustomed to use in the measures of real double stars of the same angular distance. In general each observation consists of three readings of the position-angle and two of the double distance; between the two latter readings the point of coincidence of the micrometer-wires has always been changed a little. By this means the effect of periodic errors of the screw, if there be any such, is diminished, and at the same time any bias that may be produced in the mind of the observer by the recurrence of figures approximately identical is counteracted.

The angle formed by the directions of the different artificial double stars with the line joining the two reference stars situated near the circumference of the disk, and the linear distances between their centres, have been measured several times during each period of observation; and the agreement between the measures is a very satisfactory proof that during the interval of 20 years no sensible change has taken place in the relative positions of the stars on the disk.

By examining the corrections applicable to the observed position-angles to reduce them to the true directions, M. Struve is led to the following three propositions, which have served as starting-points for all further researches on the systematic errors:—

1. The errors of the position-angles do not depend directly on

the distances, but on the visual angles which the distances subtend, as viewed with the different eyepieces, at the eye of the observer.

2. The corrections can be represented for each system of stars by a constant term, and one or several other variable terms following certain laws depending on the direction of the stars with respect to the vertical.

3. The corrections are identical for position-angles differing by 180°.

In accordance with propositions 2 and 3, M. Struve assumes for the expression of the empirical law the general form

$$\text{Corr.} = K + \alpha \sin 2\phi + \beta \cos 2\phi + \gamma \sin 4\phi + \delta \cos 4\phi.$$

where the coefficients are functions of g, which M. Struve has taken as the unit of visual angles, being the angular distance as measured by the mean magnifying-power 708, and ϕ is the angle between the direction of the stars and the vertical. From an exhaustive discussion of the observations made on the artificial stars, M. Struve arrives at the definitive formula

$$\text{Corr.} = \frac{+5^\circ\!\cdot\!2}{1+0\cdot20g^2} + \frac{4^\circ\!\cdot\!4 \sin(2\phi - 27^\circ\,13')}{1+0\cdot14(3\cdot3-g)^2} + \frac{5^\circ\!\cdot\!6 \sin(4\phi - 25^\circ\,0')}{1+0\cdot20g^2} \cdot \text{(A)}$$

Similarly from a discussion of the observations of distances of the artificial double stars the formula is deduced,

$$\text{Corr.} = \frac{+0''\!\cdot\!05(g-2\cdot0)}{1+0\cdot09(4\cdot2-g)^2} + \frac{0'\cdot15 \cos(2\phi' - 28^\circ\!\cdot\!4)}{1+0\cdot06(5\cdot2-g)^2} \;.\;.\; \text{(B)}$$

where ϕ' is the true direction of the stars referred to the vertical.

In the course of these investigations, M. Struve finds that the probable error of a single observation of position-angle of the artificial stars may be expressed by $\epsilon = 1^\circ\!\cdot\!253g^{-\frac{1}{2}}$, and that the probable error of a distance measured

with magnifying-power 413=0″·080,
„ „ „ 708=0″·051,
„ „ „ 858=0″·022.

It is thus seen that probable errors of measures of distance diminish considerably with increase of magnifying-power.

When the components of a double star are very close together, it is of course impossible to measure the distance with the micrometer; and in this case observers have recourse to simple estimations, subject probably to errors considerably different from those of true measures, and which, for this reason, should be determined separately by means of the artificial stars. M. Struve's researches in this direction give for his estimations of distance a *constant* correction = +0″·073.

Whenever the two components of a very close double star were nearly equal in magnitude, and so bright that their disks appeared to form only one oblong image, M. Struve has followed other methods for obtaining an approximate knowledge of the distance between their centres; sometimes he has measured the greatest and least diameters of the figure; at other times he has contented himself with measuring the greatest diameter, and estimating the ratio of the two diameters. Evidently in both cases it is sufficient to apply to the measures systematic corrections calculated by means of formula (B), to obtain, by a very simple computation, the true value of the distance sought.

It might be supposed that the observations of real double stars ought to furnish data by means of which one could examine to what degree the empirical formulæ, deduced from measures of the artificial double stars, increased the agreement of measures made on celestial objects by the effect of the terms in the formulæ depending on the direction of the stars with respect to the vertical. However, in the great mass of the Pulkowa measures, the increase in the agreement is not very perceptible. The reason of this is that in every case where stars have been observed at about the same hour-angle, the corrections only vary a little, so that their effect is masked under the accidental errors of isolated measures. During the first period of his observations, M. Struve has generally followed the rule to observe the same system of stars as much as possible at the same hour-angles. Equatorial or southern stars were always observed near the meridian, those of from 20° to 30° of north declination one or two hours west of the meridian, and stars further north at such western hour-angles, increasing with the declination, as enabled the observer to remain in a convenient position. M. Struve pursued this course, as, at that time, his only object was to study the relative motion of systems, or to confirm the reality of suspected movements.

It afterwards occurred to him that it would be useful to observe the same systems of stars both east and west of the meridian; and in 1862 some stars were thus observed, expressly for the purpose of ascertaining the effect of the corrections deduced from the observations of the artificial stars on the agreement of measures of position-angles and distances. The result of this investigation is that, as far as position-angles are concerned, the agreement is considerably improved by the application of the systematic corrections, and that this holds good both for stars of equal magnitude, and for pairs composed of very unequal objects, but that for the distances the improvement is much less considerable. With regard to this latter fact it is no doubt explained in great part by the comparatively large probable errors of the measured distances, so that the small calculated corrections have not much influence on the result. However, M. Struve is compelled to confess that the increase in the squares of the differences between the corrected measures and their arithmetic mean, produced, in certain cases, by the computed corrections, leaves room for some doubt as to the legitimate application of his formula, as far as the variable term is concerned (of the propriety of applying the *constant* term, M. Struve has no doubt), to the observed distances of celestial objects; and he has reason to suspect that the coefficient of the variable term in this formula depends on the position of the head of the observer, a point on which he is prepared to make further researches. The favourable result obtained for the agreement of the measured position-angles by the application of the corrections corresponding to the variable term of M. Struve's empirical formula, led him naturally to have greater confidence in the correctness of the first term of the formula, and in its application to real double stars. However, it was desirable to have a more direct confirmation of the applicability of this constant term; and this was supplied by a comparison of M. Struve's observations of the position-angles of double stars with those of other astronomers, who have from time to time visited the Observatory, and, at M. Struve's invitation, have taken part in the observations, as these, from their simple nature, require no special training. From this comparison it appears that really, taking into account the probable errors of the mean values, M. Struve's corrected position-angles agree perfectly in the mean with the position-angles measured by the other astronomers. Considering moreover, that the corrections to M. Struve's measures were deduced from observations of the artificial double stars, the true directions of which were accurately known, this circumstance is a very strong argument in favour of the doctrine that the mean of the position-angles measured by a great number of different observers is absolutely exact. Having this important fact thus prominently brought before him, it seems strange that M. Struve did not act on his knowledge of it, and have the Pulkowa double stars habitually measured by several observers. This would have saved himself a great deal of labour, and astronomers would, I think, have felt much greater confidence in the mean results of the observations.

A. W. DOWNING.

[To be continued.]

Double Stars.

LIST FOR OCTOBER.

The R.A. and Dec. are for 1880. The magnitudes and colours are those given by Σ and OΣ.

Σ 2737 = ε Equulei.
R.A. $20^{\mathrm{h}}\ 53^{\mathrm{m}}\cdot 1$, Dec. $3^{\circ}\ 50'$.

Magnitudes 5·7, 6·2, 7·1. Colours, A B yellowish, C ashy white. C was discovered by H_1, B by Σ in 1835. A B form a very close binary star, and C belongs to the system. The common proper motion is $-0^{\mathrm{s}}\cdot 011$ in R.A., and $+0''\cdot 13$ in N.P.D.

A B.

1876·44	286°·4	0″·97	Spörer.

$\frac{A+B}{2}$ and C.

1877·76	73°·4	10″·82	Flammarion.

Σ 2758=61 Cygni.

R.A. 21^h 1^m·3, Dec. 38° 8′ N.

Magnitudes 5·3, 5·9. Colours golden. Famous binary, and one of the nearest fixed stars. Proper motion +5″·11 in R.A. and +3″·23 in Dec.; parallax 0″·3136.

1877·79	116°·2	19″·76	Flammarion.
78·65	·4	19·8	Gledhill.

Σ 2760.

R.A. 21^h 1^m·9, Dec. 33° 39′.

Magnitudes 7·3, 8·1. Colours, yellowish white, ashy. Uniform rectilinear motion.

1877·83	225°·4	8″·22	Flammarion.

Σ 2822.

R.A. 21^h 38^m·8, Dec. 28° 12′.

Magnitudes 4, 5. Colours, white, bluish white.

1877·57	118°·4	3″·76	Flammarion.
78·65	·7	3·7	Gledhill.

Discovered by C. Mayer in 1777. Binary. Proper motion +0^s·016 in R.A. and +0″·26 in N.P.D. A distant star has also been measured.

1823·69	61°·3	217″·4	South.
77·57	57·8	209·5	Flammarion.

Σ 2900=33 Pegasi.

R.A. 22^h 17^m·9, Dec. 20° 15′.

Magnitudes A 6, B 9·2, C 7·9. Colour, A yellowish.

A B.

1874·91	175°·6	1″·85	Gledhill.
76·83	B invisible.		Wilson.

A C.

1877·84	330°·2	63″·5	Flammarion.

A B binary; motion rectilinear so far. C fixed.

Σ 2909 = ζ Aquarii.
R.A. 22^{h} $22^{m}\cdot6$. Dec. −0° 38′.

Magnitudes 4, 4·1. Colours, greenish white. Probably first seen double by C. Mayer in 1777. Binary. Doberck's elements are:—$T=1924\cdot15$, $\Omega=140^\circ 51'$, $\lambda=134^\circ 40'$, $\gamma=44^\circ 42'$, $a=7''\cdot64$, $e=0\cdot6518$, $P=1578\cdot33$ years.

1877·70	334°·6	3″·6	Doberck.

Σ 2912 = 37 Pegasi.
R.A. 22^{h} $23^{m}\cdot9$, Dec. 3° 49′.

Magnitudes 5·8, 7·2. Colours, white. Binary.

1873·78	119°·3	0″·5	Wilson.

Σ 2978.
R.A. 23^{h} $1^{m}\cdot7$, Dec. 32° 11′.

Magnitudes 6·8, 8. Colours, white, bluish. Binary.

1873·82	144°·0	8″·7	Gledhill.

Σ 3001 = *o* Cephei.
R.A. 23^{h} $13^{m}\cdot7$, Dec. 67° 27′·

Magnitudes 5·2, 7·8. Colours, very yellow, very blue. Binary.

	°	″	
1874·91	190·3	2·65	Gledhill.
77·74	191·7	·68	Doberck.

Σ 3062.
R.A. 23^{h} $59^{m}\cdot9$, Dec. 57° 46′.

Magnitudes 6·9, 8. Colours, yellow.

A binary in rapid motion; about 340° have been described since 1782.

Dr. Doberck's elements are:—$T=1834\cdot88$, $\omega=92^\circ 7'$, $\Omega=38^\circ 35'$, $i=32^\circ 11'$, $e=0\cdot4612$, $a=1''\cdot270$, $P=104\cdot415$ years.

The proper motion is $+0''\cdot346$ in R.A. and $-0''\cdot020$ in N.P.D.

	°	″	
1876·93	298·8	1·44	Wilson.
77·59	297·1	·48	Doberck.

α Andromedæ.
R.A. 0^{h} $2^{m}\cdot2$, Dec. 28° 26′.

Magnitudes 2, 11·2. Colours, white. Rectilinear motion.

	°	″	
1877·08	271·0	71·1	Flammarion.
78·65	271·6	71·6	Gledhill.

Σ 2.

R.A. 0^h 2^m·7, Dec. 79° 0′.

Magnitudes 6·3, 6·6. Colours, yellow. Binary.

1876·85	elongated.	Flammarion.

Σ 60.

R.A. 0^h 41^m·8, Dec. 57° 11′.

Magnitudes 4, 7·6. Colours, yellow, purple. Binary. Doberck's period is 222·4 years, Grüber's 195·2 years.

	°	″	
1875·94	147·8	......	Doberck.
77·76	150·2	5·71	Gledhill.

Σ 73.

R.A. 0^h 48^m, Dec. 22° 53′.

Magnitudes 6·2, 6·8. Colours, golden. Binary. Period 349 years (Doberck).

	°	″	
1877·02	355·9	1·28	Spörer.
77·75	354·9	·15	Doberck.

Σ 202 = α Piscium.

R.A. 1^h 55^m·8, Dec. 2° 11′.

Magnitudes 2·8, 3·9. Colours, greenish white, blue. Probably binary. Variable in brightness and colour.

1877·05	324°·0	3″·08	Spörer.

Σ 205 = γ Andromedæ.

R.A. 1^h 56^m·5, Dec. 41° 45′.

Magnitudes 3, 5, 7. Colours, golden, green, blue. A B discovered by C. Mayer in 1777. B was first seen double by OΣ in 1842.

B C.

1877·05	104°·1	0″·48	Spörer.

Most of the pairs in the subjoined list are binaries, and some of them, as will be seen from the date of the latest measure, need careful measurement again. OΣ's stars are placed in brackets [413].

Σ.	R.A.		Dec.		Angle.	Dist.	Epoch.	Obs.	Mag.	
	h	m	°	′	°	″	1800+			
[413]	20	42·6	36	43	87·4	0·66	75·70	Du.	5, 6	AB.
[416]		47·7	43	18	139·8	7·35	76·78	W.	8, 8	
2734		48·4	12	39	191·7	27·0	76·77	„	8, 9	
2763	21	3·8	16	52	292·9	16·5	52·39	Mä.	8½, 9½	
2833		46·0	8	31	340·2	9·6	51·99	„	7, 10	
2842		47·9	63	28	108·2	2·1	52·65	„	8, 11	
2849		52·1	19	40	264·0	1·4	63·91	De.	8, 10½	
2855		59·1	−2	1	297·8	27·3	52·80	Mä.	8, 9½	
2860		59·4	60	16	254·6	5·1	64·94	De.	8, 9	
2863	22	0·3	64	3	284·8	6·6	74·91	Gl.	5, 6½	
2865		1·1	69	37	181·3	18·1	63·56	De.	8½, 9	
2915		26·5	6	48	158·9	12·2	63·75	„	8½, 8½	
2941		40·1	18	36	267·0	9·7	64·58	„	7½, 10	
2947		44·9	67	56	69·9	3·3	65·27	„	7, 7	
2977	23	1·5	60	47	348·4	2·0	52·65	Mä.	7, 10½	
2989		7·2	19	20	136·6	1·7	53·05	„	8½, 10	
3021		25·4	15	33	38·5	8·3	67·29	De.	8, 9	
3024		27·3	43	11	310·1	4·8	54·87	Mo.	8, 9	
3060		59·5	17	25	115·6	4·1	59·88	Mä.	8½, 8½	
[12]	0	25·1	53	52	142·1	0·5	75·80	Du.	5½, 6	

Mr. Edward Crossley's Observatory,
Bermerside, Halifax.

JOSEPH GLEDHILL.

The Eclipse of the Sun, 1878, July 29.

I LEFT Liverpool on the 4th of July, in the White Star Line steamship 'Germanic,' in company with several other members of the Royal Astronomical Society, and after a prosperous and agreeable voyage, during which several of the party occupied themselves in taking nautical observations and working out the place of the ship, independently of Captain Kennedy, but generally in fair agreement with him, and after a very long and hot railroad journey, arrived at Denver, on July 20. Denver is situated in about N. lat. 39° 45′ and W. long. 105°, and 5200 feet above the sea-level, an elevation of which we had fine demonstration during the last night of our journey across the prairies in the uncommon brilliancy of the stars—visible on all sides down to the very horizon; whilst the Milky Way seemed absolutely clotted with light.

We found that the American astronomers had occupied the line of totality in force, of which we had agreeable evidence, from our train being visited at *Las Animas* (the first principal station after the railroad struck the line of totality) by Professors Eastman and Cleveland Abbé, who introduced themselves to us in a most cordial manner; and from these gentlemen, as well as from all others engaged in the different observing parties with whom we came in contact, we invariably received most friendly assistance and advice.

It was considered of great importance, looking at the uncertainty of the weather, that the observers should be judiciously spread about. Mr. Ranyard, with his 13 in. photographing lens, joined the camp of Professor Young on the banks of Cherry Creek, a tributary to the larger river, the Platte, which runs through Denver: at present, indeed, Denver occupies its right bank exclusively; but at the rate at which this town, just of age, is growing, the Platte will soon cease to be outside the town. Prof. Young's camp was about 2½ miles to the south-east of the town. Mr. Loder was offered the use of a garden on the southern outskirts of Denver, and he there established a fine 4-in. achromatic equatorial, his wife and brother-in-law assisting with other telescopes.

A young gentleman of Denver, Mr. H. L. Baldwin, introduced through Prof. Young, volunteered his assistance to me; and I selected a place about 2½ miles to the north-west, commanding a fine and uninterrupted view of the Rocky Mountains, and of the tract of the prairie, about 20 miles wide, which intervened between my position and the course of the shadow's approach. I could also have seen its retreat had that been required. I made this choice of ground so that, in the event of unfavourable observing weather, I might have the opportunity of seeing the terrestrial effect; and, indeed, the weather for the previous week was calculated to engender despair of seeing any thing else. Totality was due about half-past three, local time; and we had every day seen beautiful mornings followed at about 2 o'clock by thunderstorms and even on two occasions by such heavy rainfall, that the generally dry bed of Cherry Creek was changed into a torrent, and the water rose within a very few inches of Prof. Young's camp and almost threatened to make sad havoc with his beautiful and costly preparations. The splendour of the 29th was therefore the more welcome; and as the day went on and the clear sky showed no blemish, all gloomy forebodings were dissipated.

I shall not meddle with the observations of others beyond alluding to the interest felt on the evening of the eclipse in comparing notes with our own party and with many of the American astronomers —how Mr. Loder had seen a star through the corona which proved to be δ Cancri, specially looked for and seen by Mr. Burnham of Chicago—how Mr. Ranyard had succeeded admirably with two photographs with his monster camera, but had unfortunately through an accident, lost a third—not to mention the spectroscopic and polariscopic successes of Prof. Young and others. Denver, however, appeared to have failed in its search for the intra-Mercurial planet, and we did not hear of Prof. Watson's success near Rawlins, on the Union-Pacific line, until the next day.

I found the latitude of my position, by connecting it by triangulation with a standard point in Denver, to be 39° 46′ 29″ N., and the longitude 105° 1′ 43″ west of Greenwich; the elevation about 5500 ft. above the sea. My telescope, a 2¼-in. refractor by Troughton and Simms, was mounted equatorially, and I used a magnifying-power of 30 with a field of view of 75′. For time I relied on my watch, compared before and after the eclipse with a chronometer which had been rated by the electric signals which were sent from Washington. This I had done for several days past and found that the watch rate was very steady.

The effect upon the landscape looking towards that part of the Rocky Mountains from which the shadow was about to come, and especially towards one of the loftiest and most conspicuous mountains of the entire district, about 60 miles distant and not far from the central line, called Long's Peak, was not very unusual, until about 20 minutes before totality, when a strange purple-lilac colour seemed to suffuse the mountains and lower parts of the sky, the upper sky becoming a very dark warm blue.

These colours formed a remarkable contrast to the effect of a previous total eclipse I had seen—namely, that of 1870, from near Jerez, when the prevailing colour was brown, cool on the earth, and warmer in the sky; moreover, on that occasion the darkness was much greater than on that which has recently been observed. There were a few silvery-looking clouds in the direction of Long's Peak; and as the shadow reached them these clouds were suddenly reversed in colour, changing to a purplish hue, darker than the sky behind them, and then the more distant mountains were all but effaced by the increasing gloom, rather browner than the sky, but still of a purplish grey. This was a warning to leave the contemplation of the landscape and attend to the telescope. There was still a remnant of the photosphere, which became shorter and shorter, whilst it remained by comparison remarkably thick and blunted at the cusps. This collapsed suddenly without breaking into beads (like a gaslight turned right off), and immediately as I withdrew the grey screen I had been using, appeared the black orb of the Moon, according to the first impression, almost surrounded by a rosy garland, and then the corona made its appearance. On looking more narrowly at the *sierra*, which seemed of a decided red, halfway between pink and scarlet, and with some orange-coloured dots fringing the Moon, it seemed to extend more than a semicircle and to be, as was natural, most conspicuous on the eastern limb, where the Sun had just disappeared. As totality advanced, a bright prominence, with a hooked end, shot out from the opposite limb. My attention, however, was almost exclusively bestowed upon the corona. During the first minute, I occupied myself with measuring its extension on every side, and I found I could trace it in the direction of the ecliptic to a distance of about 28′ towards the east and not quite so far towards the west. I then devoted myself to the study of its northern half (the lower part, as I saw it inverted). The minimum extent of the general corona was towards the north, where there was almost a void space for about 50° between two great sweeping curves having contrary curvature, the eastern one towards the N.E., the other towards the N.W.; but the space between these was occupied by what appeared to be the most beautiful part of the corona, and consisting of bundles of rays proceeding almost radially from the Sun's centre to a distance of perhaps 12′ from the Moon's limb. They seemed to resemble spun glass, and to be of a somewhat warmer or browner grey than the rest of the corona, which had a silvery whiteness. I thought I I could perceive some movement or palpitation at their extremities; but this, if it existed, was very slight.

There was something analogous to these rays towards the Sun's south pole, but not so marked, and more mixed up with the general corona: there were also contrasted streamers in that part. Except where these radial bundles of rays appeared, the corona seemed to die off insensibly upon the sky or field upon which it was projected; but the fact is, that in the direction of the ecliptic the corona extended much further than it could be recognized in the telescope, and observers with the naked eye so saw it. I gave one hurried glance towards it myself; but as my eye had been engaged with the telescope, I did not clearly take in the corona, but obtained a most impressive view of the black orb of the Moon surrounded with its rosy garland; and I can well believe that, as far as the beauty of the spectacle is concerned, the naked-eye observers had the best of it. The corona, as seen in the telescope towards its eastern extension, and at about 10′ to 15′ from the Moon, showed the appearance of being slightly dappled, with at least four or five markings, of a darker tone than the rest. They seemed very faint, however, and I failed to detect any decided connexion or arrangement amongst them.

I will here note two points of difference between this eclipse and that of which I observed the terrestial circumstances only, near Jerez, Dec. 22, 1870. The Moon's shadow on that occasion appeared to rush violently towards the observer, like a vastly accelerated thunder-cloud; but on this its course, notwithstanding its rapidity, seemed stealthy. On that occasion the darkness was such that I had difficulty in reading a few words of very distinctly printed matter, of a type nearly as large as that employed in the footnotes of the 'Observatory.' On this occasion the diffused light was so great that I had no difficulty during totality in seeing very fine pencil-lines on the paper I was using for my memoranda. I had a lamp at hand, but made no use of it. A good deal of this difference may have been due to the partially clouded state of the sky in 1870, and the Sun's altitude, 42° on this and 30° on that occasion, and something to 4800 feet of additional elevation; but this will not, I think, nearly account for the whole difference without calling into account the presence of matter near the Sun itself, which, either from its quantity or position, was effective in reflecting more light upon the earth on this occasion.

Immediately after the reappearance of the photosphere the contrast of the light upon the ground was very remarkable; it seemed to be glowing with tender golden colour, and was attended by the curious phenomenon of wavy shadows, apparently about 9 inches wide, traversing the surface at no great distance apart, and following nearly the course of the shadow, but with a direction somewhat more southerly. Their pace was very moderate, and was by estimation about 5 miles per hour. These have been noticed before, and are considered to be diffraction-bands fringing the true shadow. Their more southerly direction would be easily accounted for by my position several miles to the east of the central line. They appear to have been seen in great perfection by General Meyer and the other observers on Pike's Peak, about 70 miles to the south of Denver, a mountain more than 14,000 feet above the sea-level; and from this elevation the contrast of colour between the shadow and the penumbra referred to above was noticed before totality as well as after.

The times I noted, reduced to Greenwich mean time, were as follows:—

	h	m	s
First contact	9	19	36·2
Second contact	10	28	56·2
Third contact	10	31	42·2
Fourth contact	11	34	56·2

The first contact was deduced from a measurement of cusps, and I do not consider it a good observation. It may be in error ± 4ˢ or 5ˢ, and is probably too late. The commencement of totality may, I think, be subject to error ± 1½ˢ. The third and fourth are, I think, pretty close.

Previous to the eclipse the wind was brisk, and I considered it necessary to provide an enclosure for the telescope; but it fell almost to a dead calm about the time of totality, but got up again as the eclipse went off. The temperature also changed in a most refreshing manner during the eclipse; but I have not preserved the record. The differences as stated at the Signal Service establishment at Denver were:—

Denver M.T.	Thermometer in the sun.	In the shade.
P.M.	°	°
2.23 (about first contact)......	114	88
3.32 (during totality)	82	84·5
4.35 (about final contact)	100	89

September, 1878. F. C. Penrose.

CORRESPONDENCE.

To the Editor of 'The Observatory.'

Tempel's Comet. 1878, August 4.

Sir,—

You will already have learnt some time ago that I succeeded at last, on July 19, after long search (since May 3!), in discovering the periodical Comet II. 1873. I send you only two observations, because I am not aware whether you already know of any others; I know of none myself*. The first observation is by Winnecke, and the second by me:—

	R. A.	Dec.
July 20^d 9^h 59^m 19^s Strass. M. T.	15^h 17^m 42·62	−4° 39′ 23″·7
30 9 50 25 Arcetri M. T.	15 30 11·92	−8 43 1·9

The comet, as seen in Amici I., is pretty large, like a nebula, of 3′ to 4′ diameter. The south preceding side (on its apparent path) is brighter and shows several small nuclei, the following side is diffused, so that it has the oval form and appearance of Encke's comet, though on the whole much smaller. A few days ago I saw it fairly well with my 4-inch Steinheil.

1878, August 19.

I hasten to inform you that I have unfortunately not made any additional observations of Comet II. 1873, and have had no news from Rome, Milan, or Padua.

Since the Full Moon I have seen the comet each day, on Aug. 16, 17, and 18. It has the same appearance as before, and yesterday evening could be very well seen with the comet-seeker of the Observatory (65 mm.=2·6 in. aperture). On Aug. 17 I made a comparison with a star 9–10 mag.; but unfortunately the latter is not to be found in any catalogue. *What a gigantic task lies before astronomers—to catalogue all stars down to the ninth magnitude! How useful it would be, and how pleasant to find a catalogued star on making comparisons!* The Comet will be visible with Amici I. only a few days longer; for the ill-planned mounting of this telescope does not harmonize with the height of the horizontal curb of the dome, so that it is not possible to see objects beyond 65° to 70° zenith distance.

Herr Schulhof writes to me that it is of extreme importance to obtain observations of the comet to the utmost limit of its visibility, since its next return will be extremely unfavourable and far south, so that even the roughest observations would be of use. We can only reckon on the next return but one.

I am, Sir, yours faithfully,

Arcetri, Florence. WILHELM TEMPEL.

Remarkable Telescopic Views of Jupiter and his Satellites.

SIR,—

In the September number of the 'Observatory, Mr. J. Rand Capron gave an account of an observation of the planet Jupiter and his satellites, made by him at Guildford, Surrey, on the evening of Wednesday, 7th August last, in which he was much struck with the beauty of its details, and concludes by saying "that there must have been something peculiar in the state of either our own or

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* Herr Tempel has since obtained five more observations.

Jupiter's atmosphere to account for the unusual bright colouring and sharpness of detail which I noticed." On the same date (13th August, 1878) as Mr. Rand Capron's communication to the 'Observatory,' I received a letter on the same subject from Mr. John Robertson, an astronomical correspondent of mine at Coupar Angus, a small town on the border of Forfarshire, in north latitude 56° 32′ 45″ and west longitude of Greenwich 3° 16′. Mr. Robertson says:—"I was delighted on the night of the 7th instant with the best view of Jupiter I have ever had. About 10^h P.M. I was struck with the appearance of the night, and after getting out my telescope, a three-inch object-glass, 45 inches focal length, by Cooke of York, the magnifying-power used being 180, I observed the fourth satellite of Jupiter just off his disk at the western limb, and the shadow of that satellite about the middle of its passage across the planet, grazing the lower edge of the lower belt. The shadow appeared much larger than the satellite, and I saw distinctly the penumbra surrounding the darker shadow, but saw no line of demarkation between them, but gradually becoming fainter towards the edge, which seemed very much of the same tint as the border of the belt along which it moved." The belts of Jupiter on this evening appear to have been more distinctly defined than is generally the case. Mr. Robertson mentions that there was a slight thunder-storm between the hours of three and five in the afternoon. There cannot be a doubt but that the atmosphere on the evening alluded to must have been in a remarkably favourable condition for telescopic observation at the same time both in the southern and northern parts of Great Britain.

I am, Sir, yours truly,

Arbroath, 17th Sept., 1878. ALEX. BROWN.

NOTES.

THE DISCOVERY OF VULCAN.—Further particulars of their observations during the late eclipse of the Sun have been given by Prof. Watson and Mr. Swift. Prof. Watson, in the 'American Journal of Science' for September, states that he mounted circles of cardboard pasted on hard wood on a four-inch equatoreal, there being no circles originally attached to the instrument. On these the pointings for the various objects observed were marked with a sharp pencil, and the designations added, so that there is no difficulty in identifying the several marks. The equatoreal having been carefully adjusted, sweeps were made east and west extending about 8° from the Sun. Prof. Watson had previously committed to memory the relative places of stars near the Sun down to the seventh magnitude. In the first sweep eastward, δ Cancri and smaller stars marked in the chart were seen. The next sweep was also eastward, but a field further south. Then placing the Sun in the field, a sweep was made to the westward, in which a ruddy star with a larger disk than the spurious disk of a star, was met with between the Sun and θ Cancri and south of the middle of the field. Its position was marked on the paper circles and designated (*a*), the place of the Sun having been recorded a few minutes previously. In the next and final sweep an object was brought into the field which was taken for ζ Cancri, though it appeared much brighter than was expected. It was designated (*b*). Prof Watson afterwards found that its position as recorded differed 4^{m} or 1° in R.A. from that of ζ Cancri, agreeing, however, within $2'$ in Declination. The telescope was clamped pretty tight in Declination, but it had a freer motion in R.A., and if it had been disturbed by the wind (from which, however, it was protected by a sand-ledge) the disturbance would probably be wholly in right ascension. In a letter to the Astronomer Royal, dated Sept. 2, Prof. Watson gives a corrected determination of the place of the objects (*a*) and (*b*), in which allowance is made for the excentricity of the hour-circle. The position of the Sun was recorded four times, once before totality at $5^{h}\,7^{m}\,31^{s}$ Washington M. T., and afterwards at $5^{h}\,22^{m}\,51^{s}$, $5^{h}\,32^{m}\,31^{s}$, and $6^{h}\,14^{m}\,36^{s}$; and these readings give a means of determining the excentricity, for which Prof. Watson gives the expression $103'\cdot7\ \sin\,(R+63^{\circ}\cdot2)$, R being the circle-reading. The probable error of the mean of ten readings of each mark is only about $0'\cdot5$, and the four determinations of $(a)-\odot$ are $8^{m}\,31^{s}\cdot6$, $8^{m}\,37^{s}\cdot6$, $8^{m}\,25^{s}\cdot6$, and $8^{m}\,31^{s}\cdot5$, with similar discordances for $(b)-\odot$.

The final result is:—

	Washington M. T.		R.A.	Dec.
	h m s		h m s	° ′
1878, July 29,	5 16 37	(*a*)	8 27 24	+18 16
	5 17 46	(*b*)	8 9 24	+18 3

The following tabular places may be useful for comparison:—

	R.A.	Dec.
δ Cancri	8 37 47	+18 36·1
⊙	8 35 56	18 38·4
θ Cancri	8 24 40	18 30·3
ζ Cancri	8 5 15	18 0·9

In the 'American Journal,' Prof. Watson had previously given the R.A. of (*a*) $8^{h}\,27^{m}\,35^{s}$. In the R.A. as first given, $8^{h}\,26^{m}\,54^{s}$, which had been hurriedly worked out, a mistake had been made in the comparison of chronometers. In a more recent letter ('Astronomische Nachrichten,' No. 2219), he gives for (*a*) $8^{h}\,27^{m}\,43^{s}$, and for (*b*) $8^{h}\,9^{m}\,18^{s}$.

Mr. Swift writes to the Astronomer Royal, under date Aug. 30:—"I was stationed at Denver, some 150 miles S.E. from Prof. Watson, and am able to confirm his observations in every particular except magnitude. During the first half of totality I had three views of the planet, and at each made careful estimates of its brightness, colour, distance from Sun, and distance and direction from θ Cancri, which was in the same field. The following is a summary of my comparisons and estimations. Distance from Sun's centre to a point midway between them =3°. Distance between planet and θ Cancri =7′ or 8′. Magnitude exactly equal, colour intensely red, and both pointed to the Sun's centre; each had large round disks, and both free from twinkling. These features were compared with each other three different times, and each time with the same result; and I am certain of them all, but unfortunately I had no means of ascertaining which was the planet and which the star. Fortunately Prof. Watson can supply this deficiency, and says, 'the one nearest the sun.'

"As soon as totality was over, I made a record of the above facts, and immediately read them to my two assistants, and to Prof. Hough, who was stationed some 20 feet from me, and at night to Mr. Burnham and many others. Having seen it stated that Prof. Watson called the Declination of the object +18°, I wrote to him asking if that was its true declination, as I could not make my observation harmonize with his—*not telling him how I wanted it.* He replied that its declination was 18° 25′, and thought it would come out a little more than that. I wanted it (to coincide with my observation) about 18° 30′. The reason why I am able to locate it so accurately with nothing to measure with, is the fact that they ranged to the Sun's centre. How much ought to be allowed for it to move in R.A. in 5^m we do not know—probably 5′ of arc. His observations make its R.A. a little more than mine; and I see no way to reconcile them with mine, but to assume that it has just passed its inferior conjunction, and that between our observations it had retrograded a few minutes of arc. In this way our observations as regards position very nearly, if not exactly, agree. My telescope was of 4½ inches aperture, power 25, field 1⅓°, comet eyepiece. At the time I estimated the two objects as bright in the telescope as Polaris is to the naked eye, and the distance between them appeared a little greater than half that between Mizar and Alcor, say about 7′, possibly 8′."

THE ECLIPSE OF THE SUN, 1878, JULY 29.—We have received a number of American newspapers with accounts of the important observations made on this occasion. The 'Chicago Times' has the credit of having fitted out an expedition under Mr. Burnham's direction, expressly for observation of this eclipse. Mr. Burnham had his six-inch equatoreal and Dr. Swazey, his companion, a four-inch refractor. They describe the corona as less brilliant than in the eclipse of 1869, the inner corona being faintly yet quite distinctly marked, and thinner on the western than on the eastern edge. The outer corona shot out in beautiful radiations broken by yellow rather than black spaces, quite evenly to a distance of 27′ from the Moon's limb, with the exception of the polar extension, which streamed off in direct lines to a greater distance, the lines on the right and left bending somewhat like a sheaf of grain. The equatorial extension, which was shorter, was also marked by lines slightly curved, and on the eastern limb had something of a mottled appearance. The star δ Cancri was seen distinctly through the corona, though within 10′ of the Sun's limb.

Prof. H. Draper gives, in the 'American Journal' for September, an account of the expedition which he organized. The party consisted of Prof. Draper and his wife, Professors Barker and Morton, and Mr. Edison; they were provided with spectroscopes, polariscopes, and photographic apparatus, the main object being to ascertain whether the corona was an incandescent gas, or whether it shone by reflected sunlight. "The results obtained were:—(1) the spectrum of the corona was photographed and shown to be of the same character as that of the Sun, and not due to a special incandescent gas; (2) a fine photograph of the corona was obtained, extending in some parts to a height of 20′, that is, more than 500,000 miles; (3) the Fraunhofer dark lines were observed by both Professors Barker and Morton in the corona; (4) the polarization was shown by Prof. Morton to be radial, that is, such as would answer to reflected solar light; (5) Mr. Edison found that the heat of the corona was sufficient to send the index beam of light entirely off the scale of the galvanometer of his tasimeter. Some negative results were also reached, the principal being that the 1474 K or so-called corona line was either very faint, or else not present in the upper part of the corona, being only seen close to the Sun. The general conclusion is, that on this occasion we have ascertained the true nature of the corona, viz.:—it shines by light reflected from the Sun by a cloud of meteors surrounding that luminary, and that on former occasions it has been infiltrated with materials thrown up from the chromosphere, notably with the 1474 matter and hydrogen. As the chromosphere is now quiescent this infiltration has taken place to a scarcely perceptible degree recently. This explanation of the nature of the corona reconciles itself so well with many facts that have been difficult to explain, such as the low pressure at the surface of the Sun, that it gains thereby additional strength."

The "photo-telespectroscope" used by Prof. H. Draper had a quadruple achromatic objective of 6 inches aperture and only 21 inches focal length, giving an image of the Sun less than one quarter of an inch in diameter and of extreme brilliancy, a most important condition. A Rutherfurd diffraction-grating 2 inches square, ruled on speculum metal, intercepted the rays before they came to a focus; and being set at an angle of 60°, threw the beam on one side, producing there three images—a central one of the Sun, and on either side of it a spectrum. One of these spectra was dispersed twice as much as the other, giving a photograph about 2 inches long in the actinic region. The photograph taken with this instrument shows a continuous spectrum, without any rings (corresponding to bright lines), such as would have been formed by an incandescent gas. The plain photograph of the corona taken with the large equatoreal (a 5½-inch refractor corrected specially for photography) shows that the great mass of the matter lies in the plane of the ecliptic, but is not arranged centrally with regard to the Sun. To the eye it extended 1½° from the Sun towards the west, while it was scarcely 1° in length towards the east. The mass of meteors, if such be the construction of the corona, is therefore, according to Prof. Draper, probably arranged in an elliptical form round the Sun. Prof. Langley saw long streamers going out from the Sun as much as 3°, or about 5,000,000 miles.

The question of polarization of the corona seems to require further investigation; for Dr. Hastings, who was with Professor Holden at Central City (40 miles west of Denver), found the plane of polarization to be *tangential* to the Sun's limb instead of radial as determined by other observers. Dr. Hastings used a 4-inch telescope, with a Savart polariscope at the eye end, having a diaphragm plate perforated with six holes varying from 3′ to 16′, any one of which could be used at will. The telescope was directed by Lieut. Very at the finder, on six points of the corona ranging from 1½′ to 50′ from the Moon's limb, and in each case the plane of polarization was found to be *tangential*. Four of the readings were taken with the dark band in the centre, and two with the bright band. An unexpected result was the powerful polarization within 1½′ of the Moon's limb.

THE ECLIPSE OF THE MOON, 1878, AUG. 12.—At Greenwich the spectrum of the Moon during the eclipse was examined by Mr. Maunder with the Great Equatoreal and the Single-prism Spectroscope. It presented a remarkable contrast with that observed in the total eclipse of 1877, Aug. 23. On that occasion the spectrum of the part in the umbra was very faint, and cut off on the violet side between *b* and F in the green, whilst to the naked eye the eclipsed part was fairly bright and of a coppery hue and the details of the surface seen with great distinctness. In the late partial eclipse, on the other hand, the spectrum was very bright (that of the uneclipsed part being only two or three times as bright) and could be traced from C to beyond *h*, though the umbra seemed to the eye to cut off the Moon completely. In the finder no details could be seen, the features being completely obliterated, except just at the edge of the shadow, which was fairly sharp. No colour was shown. The spectrum showed faintly the atmospheric band δ in the yellow, band β near C (which cut off the red end), band o (between F and G), and band G in the violet, the last two being only distinguishable as slight darkenings of the spectrum. The slit was sufficiently narrow just to divide b_2 and b_4 and the D lines. The absorption at the red end varied, being greatest in the deepest part of the shadow.

From these observations we may conclude that there was a marked absence of vapour or cloud in the part of our atmosphere through which the Sun's rays passed, the atmospheric bands in the spectrum being remarkably faint.

TRANSIT OF MERCURY, 1878, MAY 6*.—Amongst the numerous observations of this phenomenon, Prof. Langley's deserve special notice, from the attention he paid to photometric determinations, using an equatoreal of 13 inches aperture, with a polarizing solar eyepiece and Jamin photometer.

The following are his chief results:—

1. The entire disk of the planet was seen outside the Sun at about 30ˢ before external contact, on a just perceptibly lighter background.

2. The true diameter, seen under these circumstances, was estimated by aid of a glass reticule to be $\frac{1}{6}$ larger than when the planet had entered on the Sun.

3. No black drop or ligament was seen, nor any bright spot or annulus.

4. The planet was not seen black, its light was not materially less than that of some Sun-spot nuclei, but bluish grey, and measured at least 8 per cent. of mean sunlight.

5. There was unquestionable evidence of heat from the same direction.

6. The planet was darker at the centre than at the edge; but in the photometric observations this was proved beyond question to be chiefly due to minute and rapid atmospheric tremor.

Prof. Langley infers from these results that the coronal background made its presence sensible without an eclipse to at least 15″ from the limb. Further, since atmospheric glare spread 8 per cent. of sunlight over both Mercury and the Corona, while we may assume that the eye would require at least one per cent. of excess of brightness in the background to make Mercury visible, we must evidently conclude that the actual brightness of the Inner Corona exceeds (at any rate) 1-1000th of that of direct sunlight; or, in other words, the Inner Corona is several hundred times as bright as the Full Moon.

SCHMIDT'S NOVA CYGNI†.—In the 'Observatory,' No. 6, we noticed Dr. Vogel's spectroscopic observations of this remarkable star. He has now published some further results confirming generally Dr. Copeland's observation at Lord Lindsay's observatory, that the light of the star has now become monochromatic, though dissenting from his view as to the coincidence of the line with that seen in the spectrum of gaseous nebulæ. Dr. Lohse also has now given his observations and conclusions, which, though made with the same instruments at the Berlin Observatory, are quite independent of Dr. Vogel's. At first the spectrum showed both bright lines and absorption bands on a continuous background, the most conspicuous of the bright lines being coincident with the brightest line of the air spectrum, whilst three of the others (C, F, and $H\gamma$) belonged to hydrogen. Some of these faded out as the star declined in brightness, and notably Hγ and another line in the violet, whilst a line in the yellow, which had before been fainter than either of these, was still conspicuous. Finally on 1877, Oct. 25, the spectrum was found to be monochromatic, as Lord Lindsay had announced, being reduced to the line in the green (nitrogen ?).

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* Monthly Notices, 1878, June.

† Monatsbericht Königl. Akad. der Wissensch. Berlin, 1877, Dec., and 1878, April.

In speculating on the causes for the appearance of new stars and of the changes in their spectra, Dr. Lohse has been led to the following conclusions:—

1. The blazing forth of new Stars may be explained with some probability as the effect of the chemical affinity of elementary substances.

2. Through the progressive cooling of the mass of a self-luminous body, composed of vapours and gases, an atmospheric envelope is finally produced, which absorbs the light to such an extent that the star becomes invisible or only faintly visible from our Earth. When through further radiation of heat the degree of cooling is reached which is requisite for the formation of those chemical combinations which form a substantial part of the whole, the union of the elements in question will give rise to an evolution of heat and light, which suddenly makes the star again visible at a great distance for a longer or shorter time.

In his second communication on this star, Dr. Vogel states that on resuming his observations on 1877, Oct. 25, he found the spectrum nearly monochromatic, consisting of a bright line and an extremely faint continuous spectrum on both sides, which latter disappeared when the spectrum was spread out by a cylindrical lens. The star was then of the eighth magnitude. On 1875, Feb. 18, when it had fallen to 11·5 mag., the spectrum examined with an eyepiece and direct-vision prism appeared perfectly monochromatic, the star being seen as a point, and better with the prism than without, as the diffused light of the sky was thereby weakened. At the same time faint stars of 12 or 13 mag. in the neighbourhood showed linear spectra. According to Dr. Vogel, the conclusion that the star has changed into a planetary nebula seems to be inadmissible, for the wave-length of the stellar line is 4990 ± 10, as determined by Dr. Vogel before the star had become too faint for measurement of the lines in the spectrum, whilst the wave-lengths of the three nebular lines are 5003, 4957, and 4861, of which the second is the most characteristic, and this line was not seen at all when the spectrum was very bright. According to Lord Lindsay's determination, the wave-length of the stellar line on Sept. 3 was 4986, which agrees closely with Dr. Vogel's, and shows that the line could only have coincided with the first nebular line.

SUN-SPOTS AND MAGNETIC VARIATIONS *.—Dr. Wolf's observations of Sun-spots in the year 1877 (supplemented, in the case of cloudy days, by those at other Observatories) leave it doubtful

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* Astronom. Mittheilungen, No. xlvi.

whether the minimum has yet been reached, although the "relative number" of spots is slightly greater than in the preceding year. The mean relative numbers for the past eleven years are:—

1867	1868	1869	1870	1871	1872	1873	1874	1875	1876	1877
7·3	37·3	73·9	139·1	111·2	101·7	66·3	44·6	17·1	11·3	12·3

But when the following means for the individual months from July 1876 to June 1877 (smoothed by means of a curve),

11·7 11·9 10·8 10·6 11·8 13·0 13·1 12·6 12·7 12·7 12·7 12·6 12·5

are examined, it appears that the rise at the beginning of 1877 has not been maintained, and that the minimum must be fixed at some later epoch, provisionally at 1878·0.

With regard to the discussion between Mr. Broun and M. Faye as to the connexion between magnetic variations and sun-spots, Prof. Wolf disputes M. Faye's statement that the period of magnetic variations is not equal to that of sun-spots as found by Mr. Broun, the fact being that the last-named observer has deduced a period of 10·45 years for both phenomena, whilst Prof. Wolf himself found 11·11 years equally for both, the probable error of this last determination being 0·3 year. Both Prof. Wolf and Mr. Broun are agreed that there is coincidence between the two phenomena, continued throughout the whole 90 years of available observations, but they differ as to their treatment of some of the earlier and imperfect records. Prof. Wolf maintains that to convert his minimum of 1775·5 into Mr. Broun's maximum of 1776·2 the series of observations of Sun-spots by Standacher, Horrebow, Mallet, &c., which mutually support and supplement each other, must be rejected as false; whilst to obtain Mr. Broun's minimum of 1795 and maximum of 1797 the same treatment must be applied to Flaugergues.

As regards the magnetic variations, Prof. Wolf has rediscussed all records from 1783 to 1877, a period which includes, on his view, 9 minima and 8 maxima; whilst Mr. Broun obtained 10 minima and 9 maxima by adopting a slight minimum in 1795·0 and maximum at 1797·7 between the maximum of 1787·2 and minimum of 1799·0. These two intercalated epochs rest, in Prof. Wolf's judgment, on insufficient data, the observations being far from complete. The following are the epochs of minimum and maximum magnetic variations (*v*) adopted by Prof. Wolf, with their discordance from the epochs (*v'*) calculated on the assumption of a constant period (11·14 years), and their discordance from the epochs of observed minimum and maximum of sun-spots (*s*):—

Min.	1784·5	1799·0	1811·0	1824·2	1833·8	1844·5	1856·4	1866·8	1877·5
v−*v'*	−4·5	−1·2	−0·3	+1·8	+0·2	−0·2	+0·5	−0·2	−0·6
v−*s*	−0·2	+0·7	+0·4	+0·9	−0·1	+1·0	+0·4	−0·4	−0·5

Max.	1787·2	1804·0	1818·2	1829·7	1837·7	1848·7	1859·8	1870·8
v−*v'*	−6·1	−0·4	+2·7	+3·0	−0·1	−0·3	−0·3	−0·4
v−*s*	−0·9	−0·2	+1·8	−0·2	+0·5	+0·6	−0·3	+0·2

From these epochs Prof. Wolf has deduced four slightly different values of the mean period according as all are used or as the first two are rejected, and according as equal or unequal weights are given. The values thus found are 11·77, 11·18, 11·04, and 11·03, the mean being 11·14 years, with a probable error of 0·22 year and an average variation in the individual period of 1·93 year on either side, whilst the average discordance $(v-s)$ from the sun-spot curve is only 0·62 year.

Prof. Wolf points to this fact that the mean value of $v-s$ is so much less than that of $v-v'$, as conclusive evidence of the parallelism between magnetic variations and sun-spots. The values found for the two periods are almost identical, being 11·14 and 11·11 years respectively.

The mean epoch of minimum is 1833·59, and of maximum 1837·83; so that a maximum follows a minimum at an interval of only 4·24 years, agreeing closely with the abrupt rise of the Sun-spot curve towards a maximum (*cf.* 'Observatory,' Vol. i. p. 24).

PROF. ÅSTRAND'S STAR-CHART.—In a single map, folding in a space of about 9 inches by 4, Prof. Åstrand has laid down some 4000 stars, included between 35° S. and 80° N. Declination, a work which he has executed with the greatest care in the hope that it may prove useful to amateur astronomers, and especially to meteor observers, as from its compactness it cannot fail to do. There are several novelties introduced into this little map which have enabled Prof. Åstrand to represent so many stars with less confusion than is to be found on many maps of far larger size. In the first place the projection chosen appears to have great convenience. The stars are supposed to be projected on a tangent cone to the sphere having its apex at the north pole, the vertical angle being 30°, so that it touches the sphere at 30° N. declination; and there is, on this projection, no very great distortion within the limits specified (80° N. to 35° S.), except for the extreme southern portion. The stars are thus represented on a semi-circle 22 inches in diameter, with the central part near the pole, which would be much distorted, cut away, the cone being cut through along the line of 0^h R.A., and opened out or "developed."

For convenience in use the map is folded into segments of 3^h in R.A., and the two portions on each side of 0^h can be brought together. This offers the most convenient arrangement for following the constellations that we have seen, for the map is thus continuous, and the awkward breaks which are so annoying in ordinary maps are entirely avoided. A small polar map is all that is required to complete it.

Further, the stars are printed in gold on a dark purple background, which is highly glazed, and thus shows up the bright dots with great distinctness, considering how crowded the map is. Possibly Prof. Åstrand has endeavoured to represent a little too much, for he gives 1000 stars more than Argelander in his beautifully engraved 'Uranometria,' and to nearly every one of them a letter or number.

If the latter had been omitted, the map would undoubtedly have gained in clearness; the configurations of the principal stars, however, are sufficiently marked. But a supplementary map showing the stars without any names or numbers would have been still more acceptable, and the present map might then have served as a key-map. The idea of this map is so good that we cannot help offering this suggestion, in the hope that it may lead Prof. Åstrand to make his work still more useful for the identification of the stars. It will prove specially valuable to meteor observers, as the tracks can be laid down with a soft red pencil, and afterwards rubbed out without injury to the glazed paper. Its compactness leaves nothing to be desired.

M. ERNEST QUETELET.—We are sorry to have to record the premature decease of this astronomer, the well-known son and assistant of the late distinguished director of the Brussels Observatory. He was born at Brussels on the 7th of August, 1825, and he received his early education at the École Militaire, from which he passed in due course into the Engineers. He was frequently engaged with his Corps on Scientific work, and at one time he was employed on the works of the extensive fortifications around Antwerp. His taste for observatory work, and a desire to be of assistance to his father, encouraged him to retire from the Engineers, and to become in 1855 an Assistant at the Brussels Observatory, where he at once took an active part in the daily observations, the direction of which, owing to the advancing age of his father, practically fell upon him. Most of his contributions on astronomy and magnetism may be found in the "Mémoires" and "Bulletins" of the Académie des Sciences of Brussels, among which we may mention his papers on the magnetism of the Earth in Northern Germany and Holland; on the magnetic declination and inclination observed at Brussels; on the proper motions of certain stars; and on the revision of the list of the variable stars. Owing to symptoms of failing health, of which possibly he was alone conscious, he was led to shrink from the cares of the permanent direction of the Brussels Observatory after the death of his father in 1874. M. Quetelet was fifty-three years of age at the time of his decease, which occurred at Ixelles on September 6. E. D.

DR. E. VON ASTEN.—We regret to announce the death, on Aug. 15, at Kiel, of this able astronomer, at the early age of thirty-six. Dr. Von Asten was attached to the Pulkowa Observatory, where he undertook the important researches on the motion of Encke's comet, of which an account was given in the 'Observatory,' No. 1. The premature death of this able investigator will be felt as a severe loss to gravitational astronomy.

Mr. Thomas Grubb, F.R.S.—We are sorry to hear of the death of this eminent optician on Sept. 19, in the 78th year of his age. Mr. Grubb was a self-taught mechanician, and after turning his attention to machinery for printing bank-notes and other industrial purposes, he took up the manufacture of optical instruments, a work in which he achieved great success, being ably seconded by his son. He has of late years undertaken the construction of some of the largest reflectors and refractors of the world, including the great Melbourne reflector.

Meteors.—Mr. Bassett, writing from Holdsworthy, Devon, mentions that he saw two bright meteors, one following the other at a little distance, on August 16, about 7.15 P.M. They travelled at great speed in a south-westerly direction, and disappeared behind storm-clouds which were then gathering in the horizon. They remained visible for a considerable time, estimated by several persons as from 1½ to 2 minutes; but this was a mere guess, and is probably enormously in excess, unless it refers to luminous *streaks* left by the meteors.

Discovery of two Minor Planets.—A new planet (No. 189) was discovered by Prof. Peters at Clinton, N. Y., on Sept. 17; and another (No. 190) by Prof. James C. Watson at Ann Arbor on Sept. 22. Both planets are of the eleventh magnitude.

Editor.

Meteor Notes for October.

The first and last ten days of the month are chiefly available for observations (in the absence of the Moon). The *Orionids* return on October 18–20, and there are many showers of less intensity occurring at this period. The *Taurids* I. begin at about the middle of the month, and, so far as it is possible to deduce from existing records, continue until the end of November. The major radiant-point is at 62° +21°, and probable maximum on Nov. 20–23; but this shower is also conspicuous on about October 17–22, and during the first few days of November. Two other radiants lie close to it, one at 56° +24° (Nov. 1–15), and the other at 53° +16° (well seen by Capt. Tupman on Nov. 7–9, 1869). There are also good showers at 77° +32° and 81° +23° in October. The former was well seen by the writer on Oct. 8, and again on Nov. 7 last year. Two showers of bright slow-moving meteors were also traced on Oct. 2–5, at 290° +70° and 253° +64°. On the 8th there is an active radiant of swift meteors from 47° +28° (noted in 1877). This is the same as one observed the previous year at 46° +26° (19 meteors) Sept. 20–Oct. 25, but is quite distinct from a rich display of slow bright meteors seen in 1877 from 43° +22°, Oct. 28–Nov. 1. Dr. Weiss's collection of meteor paths, observed in Austria during the years 1867–74 indicate a radiant at 34° +52° for Oct. 20–31, which confirms a shower of slow meteors seen at Bristol on the 8th at 32° +50°. Heis's radiant P_1 at 24° +42° (83 meteors), Oct. 17–31, should also be mentioned as visible in the evenings. Looking eastwards after midnight, the observer will trace many showers from Leo, Cancer, Canis minor, &c. The *Gemellids* form an important display in October, with a centre at 108° +24° (Greg 105° +27°). Other tolerably active systems appear at 133° +79°, 105° +50°, 107° +12°, 130° +46° (all of very swift streak-leaving meteors); and on the mornings of the 15th–18th last year, three very distinct and certain showers were seen at 133° +21°, 140° +28°, and 120° +15°, and it will be interesting to recover these as soon as the waning moon allows after about the 20th. The visible duration of all these radiant-points is utterly unknown. No doubt is entertained as to the actual existence of the showers, but the nights on which they begin and end remain in obscurity. The whole attention of observers is now required to this question of *duration*; for until the limits of visibility of certain showers are determined, it is impossible to distinguish or separate a new shower from an old one occurring from the same point with a few weeks difference of epoch. The three showers last mentioned each apparently endure until the end of December; for in the meteor catalogues of Heis, Weiss, Zezioli, Schiaparelli, and others are a vast number of paths for the periodical shower dates of Oct. 18–20, Nov. 10–15, Nov. 27, Dec. 9–12, &c., some of which on being projected are resolved into exactly the same centres of radiation, and the writer's own observations amply confirm the results obtained from other sources. The work of further investigating this singular point will require great care. All doubtful paths should be rejected and the foreshortened and accurately observed meteors chiefly relied upon. It is very important to get the *directions* exactly. When a meteor is seen it is a good plan to project immediately a straight rod (held in the hand for the purpose) upon its apparent path, and then carefully note the direction with reference to stars near. This method will often give results of much precision, and it is obviously preferable to relying solely upon the eye, which always fails to retain the direction accurately, and sometimes gives a second erroneous impression while the observer is attempting to fix the real path by the stars. Moreover, when two meteors fall almost simultaneously, as they often do, the application of this "direction-rod" enables them both to be registered with fair precision; and the plan is evidently very good in the case of slow meteors, or of meteors leaving streaks.

Recent Observations.—Mr. E. F. Sawyer, of Boston, Mass., writes that his observations of the *Perseids* were this year seriously interfered with by moonlight and cloudy weather on Aug. 8–11. During a clear interval, however, of $1^h\ 45^m$ on the 9th from 14^h to $15^h\ 45^m$ he noted 98 meteors, of which 58 were *Perseids*, and he deduces the horary number of *Perseids* at 33, of all meteors 56. The colour of the brighter *Perseids* appeared to be either orange or yellow. The radiant-point was determined from one stationary meteor of the third mag., accurately noted at 44° +56°·5, and from several short paths near that centre. A shower from 8° +55°, between α and ξ Cassiopeiæ, was also observed.

Mr. H. Corder's observations at Chelmsford in July–Aug, 1877–8, gives radiants at 328° +47°, 290° +67°, and 353° +43°. The two former verify streams seen this year at 332° +50° and 291° +70° (see last month's "Notes"). The latter position also agrees with a shower seen this year by the writer at 354° +42° (7 meteors), July 26–27.

Several fine meteors are reported by Mr. V. Cornish at Debenham. On Aug. 7, 10^h 27^m, one = ♃ was seen, with a path from 12° +60° to 10° +45°. It left a streak. Two minutes later another meteor, twice as bright as ♃, took a course (not well observed) from 335° +26° to 335° +12°. At about the same time a third meteor (seen by another observer) = ♃ shot from 320½° −5° to 328¾° −1°. On Aug. 20, 8^h 57^m, an observer, while walking from Yarmouth to Lowestoft, saw a meteor = ♃ pass from 279° −4½° to 275° −24°; and at 13^h on same night a bright meteor was observed at Debenham, beginning in due S. alt. 45°, and passing to westward a distance of 25°. On Aug. 27, 9^h 32^m, a fine meteor began at 339° +18° and ended at 338½° +14°. On Aug. 31, 11^h, a fireball equal 1-3rd ☽ began a little W. of S., and travelled in a distinctly curved path to S.E. by E. [Altitude not stated.] It lit the place up. At 11^h 20^m that night another meteor of smaller size was seen following the same direction.

At Bristol a fine meteor nearly = ♀ was seen by the writer at about 10^h 20^m, Sept. 1. It fell from 161° +70° to 155°+56°. Very swift; short streak at 156° +58° for 10 sec. A meteor brighter than ♀ was seen in the moonlight at 8^h 35^m on Sept. 8, going from 246° +21° to 244½° +5°. Very swift; streak of 4° for 25 sec. These paths prolonged back intersect at 295° +83°, and the two meteors having been very similar in appearance were probably members of a common radiant-point there. The shower N_{14}, of Heis, Aug. 11–31 (96 meteors), at 295°+ 79°, is no doubt the same, and a radiant at 300° +82°, for Aug. 24–Sep. 14 (14 meteors), deduced from the Italian observations 1872, agrees with it well; but we require one or more duplicate observations of the two bright meteors described, before the radiant can be certainly fixed. The evening of Sept. 1, when the first was seen, was noted to be very rich in shooting-stars, for during the five minutes preceding 10^h 25^m, 8 were observed, and at other times they were noted in a casual way to be unusually numerous; but there was great variety in their directions, and hence they must have belonged to many contemporaneous systems.

W. F. Denning.

Oct.	G. M. T.	PHENOMENA.
	h m	
16. W.	7 34	♃ 3 Sat. Oc. R.
	6 21·3	♃ tr. D. 21° 13′ S.
	10 14·4	♄ tr. D. 3 15 S.
	10 18·8	♈ transits.
	10 29	♃ sets.
	12 46·5	♆ tr. D. 12 42 N.
	16 2	♄ sets.
	18 29	☉ rises.
	23 1·8	♀ tr. D. 3 16 S.
	23 28·3	☿ tr. D. 6 10 S.
17. Th.	23 45·4	☉ tr. D. 9 19 S.
	5 2	☉ sets.
	7 0	Twilight ends.
	6 33	♃ 2 Sat. Sh. E.
	9 17	ε Gem. Oc. D. 79°
	10 9	ε Gem. Oc. R. 283°
18. F.	8 15	♃ 1 Sat. Tr. I.
☾	19 9·6	
19. S.	13 47	δ Canc. Oc. D. 42°
	14 14	δ Canc. Oc. R. 352°
20. S.	6 24	♃ 1 Sat. Sh. E.
	19 7	18 Leo Oc. R. 328°
21. M.	23 5·2	♀ tr. D. 5° 44′ S.
	23 39·7	☿ tr. D. 9 46 S.
22 Tu.	8 24	♃ 2 Sat. Oc. D.
23. W.	7 58	♃ 3 Sat. Oc. D.
24. Th.	6 14	♃ 2 Sat. Sh. I.
	6 28	♃ 2 Sat. Tr. E.
	8	☿ Sup. ☌ ☉
	18	☾ Perigee.
25. F.		
●	10 58·4	
26. S.	7 31	♃ 1 Sat. Oc. D.
	5 45·6	♃ tr. D. 21° 2′ S.
	9 32·9	♄ tr. D. 3 28 S.
	9 39·5	♈ transits.
	23 8·7	♀ tr. D. 8 9 S.
	23 50·9	☿ tr. D. 13 9 S,
27. S.	23 44·0	☉ tr. D. 12 51 S.
	5 59	♃ 1 Sat. Sh. I.
	7 0	♃ 1 Sat. Tr. E.
	7 1	♃ 3 Sat. Sh. E.
	8 20	♃ 1 Sat. Sh. E.
28. M.	5 37·3	♃ 1 Sat. Ec. R.
30. W.	4 2	σ Sagitt. Oc. D. 109°
	5 7	σ Sagitt. Oc. R. 222°
	8 17	♃ 4 Sat. Sh. E.
Oct.	G. M. T.	PHENOMENA.
	h m	
31. Th.	6 10	♃ 2 Sat. Tr. I.
	10	♅ ☍ ☉
	23 12·6	♀ tr. D. 10° 30′ S.
Nov.		
1. F.	0 2·1	☿ tr. D. 16 14 S.
☽	9 50·7	
2. S.	5 56·4	♃ 2 Sat. Ec. R.
3. S.	5 44	♃ 3 Sat. Tr. E.
	6 37	♃ 1 Sat. Tr. I.
	7 21	♃ 3 Sat. Sh. I.
	7 55	♃ 1 Sat. Sh. I.
4. M.	7 32·6	♃ 1 Sat. Ec. R.
5. Tu.	8 0	8276 Oc. D. 47°
	10 56	22 Pisc. Oc. D. 21°
	5 11·1	♃ tr. D. 20° 47′ S.
	8 51·8	♄ tr. D. 3 38 S.
	9 0·2	♈ transits.
	9 22	♃ sets.
	11 25·8	♆ tr. D. 12 32 N.
	14 36	♄ sets.
	19 5	☉ rises.
	23 16·8	♀ tr. D. 12 44 S.
6. W.	23 43·8	☉ tr. D. 16 3 S.
	0 13·5	☿ tr. D. 18 58 S.
	4 23	☉ sets.
	6 20	Twilight ends.
	9	☾ Apogee.
8. F.	17	☿ Aphelion.
9. S.	3 46	μ Ariet. Oc. D. 86°
○	14 33·9	
10. S.	5 51	9 Tauri Oc.† 161°
	6 18	♃ 3 Sat. Tr. I.
	9 31	17 Tauri Oc. D. 70°
	10 42	16 Tauri Oc. R. 292
	10 46	17 Tauri Oc. R. 249
	11 11	η Tauri Oc. D. 124
	11 21	20 Tauri Oc. R. 292
	12 2	η Tauri Oc. R. 201
	23 21·5	♀ tr. D. 14° 51′ S.
11. M.	0 25·2	☿ tr. D. 21 19 S.
	5 55	♃ 1 Sat. Oc. D.
12. Tu.	5 24	♃ 1 Sat. Tr. E.
	6 40	♃ 1 Sat. Sh. E.
13. W.	16 12	ε Gem. Oc. D. 81°
	17 21	ε Gem. Oc. R. 309
14. Th.	8 42	58 Gem. Oc. R. 227

† A near approach.

EDITOR.

DISTANCES from Saturn's polar diam., in equatoreal semidiams.
— preceding or west, + following or east, N. north, S. south.

12^h G.M.T.	Encel.	Tethys.	Dione.	Rhea.	Titan.
Oct. 16....	−4·0 N	+1·6 S	−0·4 N	−8·8 N	+ 2·1 S
17....	+0·7 N	−2·5 N	−4·5 S	−3·0 S	+ 9·9 S
18....	+3·9 S	+3·3 S	+6·4 S	+7·7 S	+16·2 S
19....	−1·7 S	−3·9 N	+4·0 N	+6·8 N	+20·2 S
20....	−3·4 N	+4·4 S	−1·1 S	−5·7 N	+21·3 N
21....	+2·5 N	−4·8 N	+5·5 N	−7·8 S	+19·4 N
22....	+2·8 S	+5·0 S	−6·1 N	+2·9 S	+14·7 N
23....	−3·2 S	−5·0 S	+2·7 S	+8·8 N	+ 7·9 N
24....	−2·0 N	+4·8 N	+2·6 N	+0·3 N	− 0·1 N
25....	+3·7 N	−4·5 S	−6·1 S	−8·7 N	− 8·0 N
26....	+1·0 S	+4·0 N	+5·5 S	−3·4 S	−14·0 N
27....	−4·0 S	−3·4 S	−1·2 N	+7·5 S	−19·0 N
28....	0·0 N	+2·6 N	−3·9 S	+6·1 N	−20·1 S
29....	+4·0 S	−1·8 S	+6·4 N	−5·4 N	−17·8 S
30....	−1·0 S	+0·9 N	−4·6 N	−8·0 S	−12·6 S
31....	−3·7 N	0·0 S	−0·3 S	+2·5 S	− 5·3 S
8^h G.M.T.					
Nov. 1....	+3·8 N	+1·7 N	+6·1 N	+8·9 S	+ 1·5 S
2....	+0·7 S	−0·9 S	−5·4 N	+2·7 N	+ 9·3 S
3....	−4·0 S	−0·1 N	+1·0 S	−7·9 N	+15·8 S
4....	+0·3 N	+1·0 S	+4·0 N	−5·5 S	+20·0 S
5....	+3·9 S	−1·9 N	−6·4 S	+6·0 S	+21·3 N
6....	−1·3 S	+2·7 S	+4·5 S	+7·6 N	+19·6 N
7....	−3·6 N	−3·4 N	+0·4 N	−3·2 N	+15·2 N
8....	+2·2 N	+4·1 S	−5·1 S	−8·8 S	+ 8·5 N
9....	+3·1 S	−4·5 N	+6·3 S	+0·1 S	+ 0·6 N
10....	−3·0 S	+4·8 S	−3·2 N	+8·8 S	− 7·4 N
11....	−2·3 N	−5·0 N	−1·9 S	+3·0 N	−14·2 N
12....	+3·6 N	+5·0 N	+5·8 N	−7·7 N	−18·7 N
13....	+1·4 S	−4·8 S	−5·8 N	−5·8 S	−20·1 S
14....	−3·9 S	+4·4 N	+1·9 S	+5·6 S	−18·1 S

Semiaxes of orbits expressed in semidiameters of Saturn's equator.

Nov. 1.	Mi.	En.	Te.	Di.	Rh.	Ti.	Iap.
Major	3·14	4·03	4·99	6·40	8·93	20·71	60·34
Minor	0·11	0·14	0·17	0·22	0·31	0·64	8·23
Hourly Motion	15°·9	10°·9	7°·9	5°·5	3°·3	0°·9	0°·2

Iapetus is near its inferior conjunction, north of the planet, on Oct. 11, near its greatest western elongation on Oct. 31, and near its superior conjunction, south of the planet, on Nov. 19.

A. M.

Satellite of Neptune.

Semi-major and minor axes of apparent orbit on Nov. 1, 17″·0 and 5″·8. The satellite is near its greatest *n.f.* elongation in pos.-angle 40°·9, and near its greatest *s.p.* elongation in pos.-angle 220°·9 at the following times:—

Oct. 17	1^h	*n.f.*	Oct. 25	21^h	*s.p.*	Nov. 3	17^h	*n.f.*	Nov. 12	12^h	*s.p.*
20	0	*s.p.*	28	20	*n.f.*	6	15	*s.p.*	15	11	*n.f.*
22	22	*n.f.*	31	18	*s.p.*	9	14	*n.f.*	18	9	*s.p.*

A. M.

Position of the Sun's North Pole.

1878.	Pos.-angle. ° ′	Dist. from limb. ° ′
Oct. 17	26 18	5 33
22	25 56	5 7
27	25 22	4 39
Nov. 1	24 36	4 9
6	23 38	3 37
11	22 28	3 3

The Moon.

1878.	Rises. h	m	Geoc. Libr. at midnight. long. °	lat. °	1878.	Sets. h	m	Geoc. Libr. at midnight. long. °	lat. °
Oct. 16...	7	17	−6·40	−4·54	Nov. 1...	10	26	+6·99	−0·21
17...	8	18	6·90	3·39	2...	11	41	6·06	1·61
18...	9	32	7·13	2·03	3...	12	51	4·90	2·90
19...	10	52	7·01	−0·53	4...	14	1	3·63	4·06
20...	12	16	6·49	+1·05	5...	15	9	2·28	5·03
21...	13	40	5·53	2·61	6...	16	18	+0·95	5·78
22...	15	7	4·14	4·05	7...	17	28	−0·32	6·28
23...	16	37	2·36	5·24	8...			1·50	6·51
24...			−0·33	6·09	9...	Full.		2·58	6·46
25...	New.		+1·80	6·50	10...			3·54	6·10
26...	Sets.		3·81	6·46	11...	Rises.		4·37	5·46
27...	4	54	5·53	5·98	12...	5	14	5·07	4·56
28...	5	41	6·81	5·12	13...	6	12	5·61	3·41
29...	6	41	7·58	3·98	14...	7	23	5·99	2·07
30...	7	52	7·84	2·65	15...	8	39	6·14	−0·61
31...	9	9	+7·62	+1·23	16...	10	1	−6·03	+0·93

The Pleiades are occulted by the Moon on November 10.

A. M.

The Planets.

Mercury is in superior conjunction with the Sun on Oct. 24.

Venus is a morning star, near superior conjunction, rising 1¼^{h} before the Sun on Oct. 15, and 30^{m} on Nov. 15. Her disk is gibbous, the illuminated portion being 0·976 on Oct. 15, and 0·997 on Nov. 15, and the corresponding diameters 10″·0 and 9″·8.

Jupiter is an evening star, in quadrature on Oct. 20. His equatoreal diameter is 37″·9 on Oct. 15 and 34″·4 on Nov. 15.

Saturn is visible till near sunrise, about midway between β Ceti and α Pegasi, moving slowly *s. p.*

	Outer Ring. Maj. Axis.	Outer Ring. Min. Axis.	Inner Ring. Maj. Axis.	Inner Ring. Min. Axis.	Ball. Diam.
Oct. 12	43″·64	1″·89	29″·02	1″·26	17″·4
Nov. 1	42″·78	1″·47	28″·45	0″·98	

The south side of the rings is visible, the elevation of the Earth above their plane being 2° 29′ S. and 1° 58′ S. on Oct. 12 and Nov. 1 respectively, and of the Sun 3° 42′ S. and 4° 0′ S.

EDITOR.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 19. NOVEMBER 1. 1878.

Binary Stars.—Part IV.

τ *Ophiuchi.*

When W. Herschel discovered τ Ophiuchi in 1783, he considered it the closest of all his double stars; and when reexamined by W. Struve in 1825 and J. Herschel in 1830, it did not present any trace of duplicity. Struve, however, in 1827 perceived an elongation in the direction 326° or 146°, and he ascribed this partly to the effects of refraction on this (especially for the high latitude of his observatory) very low star, and partly to the reappearance of the companion. For ten years after it continued one of the most difficult objects, but it since then has opened gradually, and is not now so very difficult. The components are of the fifth and sixth magnitude, both yellow. I determined the provisional elements by a repeated application of Herschel's method. At first I derived the period from observations since 1835 alone; but I subsequently altered this element in connexion with the longitude of the node, so as also to represent W. Herschel's position-angle. In these calculations I was only able to make use of about thirty annual means, so I proceeded to correct the result after Klinkerfues's second method, when I subsequently came into possession of sixty-six annual means, extending up to 1877 instead of up to 1870, as in the former case. However, the case presented such difficulties that the elements could not be corrected by this method so as to represent the six angles. I then took the three distances corresponding to the three last angles into account according to the same method, thus forming five equations of condition. But it was plainly seen that no reasonable correction of e, P, and T would satisfy these differential equations. I had then nothing left but to vary the three elements successively until the three first equations, after many unsuccessful attempts, were closely represented. Next followed the correction of the elements that fix the situation of the ellipse, and at last the axis major was derived from the distances.

Elements of τ Ophiuchi = Σ 2262.

☊	65° 26′	e	0·6055
λ	41 24	P	217·87 years
γ	58 42	T	1821·91
		a	1″·40

λ *Ophiuchi.*

λ Ophiuchi is another close and difficult double star, discovered by W. Herschel in 1783. The principal star is of the fourth magnitude and pale yellow, the companion of the sixth and bluish white. Herschel gave the angle as 75°·5 in 1783, and as 69°·3 in 1802; but later observations have established beyond doubt a direct motion from 331° in 1825 to 33° in 1877, the distance at the same time increasing from a second to a second and a half. It became thus evident that one of Herschel's angles was quadranted wrong, and the orbit was computed at different times on the assumption that the second angle should be increased 180°, an hypothesis first started by Struve. The orbits computed by Mädler and by Hind do not at all represent the later observations; and Seeliger, who has represented these, has been obliged to exclude Herschel's first angle. My orbit was founded on the whole observational material; but I increased Herschel's first angle one quadrant, besides altering the second two quadrants. The provisional elements were determined by Herschel's method, and λ, γ, and *e* subsequently corrected from differential equations, obtained by variation of these elements, while P and T were obtained from the first and the last position-angle. ☊, λ, and γ were then corrected by equations of condition; but the two first could not well be separately determined, the respective coefficients being nearly proportional. The mean anomalies were next computed for epochs corresponding to a number of normal places, and P and T obtained from them. Now I repeated the determination of the situation of the ellipse, but the corrections to the elements obtained above amounted only to a few minutes of arc. The elements being still unsatisfactory, I proceeded on Klinkerfues's second method. However the three equations only taught me that the mean anomalies had better be lessened about 7°; and I encountered similar difficulties to those which have been described under τ Ophiuchi. The relative weights of the normal places were found by trial by the final derivation of the elements that fix the situation of the ellipse.

Elements of λ Ophiuchi = Σ 2055.

	° ′		
☊ ……	157 21	*e* ……	0·4930
λ ……	94 16	P ……	233·89 years
γ ……	44 44	T ……	1803·91
		a ……	1″·19

44 *Boötis.*

44 Boötis was discovered by Herschel in 1781. He remarks that the components, with a power of 227, seemed almost to touch. They are both between the fifth and sixth magnitude and nearly white. The system has a large proper motion. The distance increased rapidly, but the nature of the change of the angle troubled

astronomers for a long time. Sir J. Herschel thought that an orbit passing nearly through the eye and so situated that the longer axis of the ellipse into which it is projected shall form an angle of about sixty degrees with the meridian, would account for "the remarkable jump the small star seems to have made from one side to the other of the large one." In 1833 he added that he had no longer any doubt that it would prove a very remarkable binary star; and says, "granting a single mistake of 180° in one of my father's measures, the history of its angles of position is consistent with an elliptic motion performed in a period of moderate length in an orbit highly inclined to the line of sight." I excluded the angle 62° 57′ for 1802, as I found it altogether different from that given by the interpolating curve; but a writer in 'Nature' has since explained it to be 207° 3′, and given a proof of the accuracy of the elements to which I arrived, as he, according to them, obtains 206° 55′ for the same epoch. The first elements were obtained graphically after drawing tangents to the interpolating curve; but the latter part of the curve is nearly horizontal, and I did not therefore attribute much weight to the distances obtained from the tangents. In the second orbit the distances observed after 1820 were used after they had been corrected by an interpolating curve, but both these orbits failed to represent the observations. I attributed this to the circumstance that the interpolating curve between 1790 and 1820 was drawn by a pure guess; and I therefore drew an ellipse of great eccentricity through the projected points corresponding to observations made between 1820 and 1875, epoch and period being derived from Herschel's angle and a normal place for 1870. The result was not satisfactory, but I obtained the elements given below by interpolations between the three orbits which I had constructed.

Elements of 44 Bŏotis = Σ 1909.

	°	′		
☊	65	29	e	0·7100
λ	1	18	P	261·12 years
γ	70	5	T	1783·01
			a	3″·093

η *Cassiopeiæ.*

η Cassiopeiæ was first observed by Herschel in 1779. The components are of fourth and seventh magnitude, yellowish and purple. I obtained the first orbits by Herschel's method, employing the distances both as they followed from the tangents and as they followed from the observed distances according to an interpolating curve. The third orbit was constructed by drawing an ellipse between the two series of points, the difficulty being in this case the want of accuracy of the older distances. The orbit was obtained by interpolations between these different systems of elements.

Elements of η Cassiopeiæ=Σ 60.

	°	′		
☊	39	57	e	0·5673
λ	223	20	P	222·435 years
γ	53	50	T	1909·24
			a	9″·83

μ^2 *Boötis.*

μ^2 Boötis was discovered by Herschel in 1781, when its components, which are both about the eighth magnitude and bluish or greenish, were a couple of seconds apart. But the measures soon became difficult from the rapid diminution in its distance. Its proper motion agrees with that of μ Boötis. The orbit was obtained exclusively from the angles, the distances being only used in deriving the axis major. I employed Herschel's method repeatedly, and followed up with equations of condition.

Elements of μ^2 Boötis=Σ 1938.

	°	′		
☊	173	42	e	0·5974
λ	29	0	P	280·29 years
γ	39	57	T	1863·51
			a	1″·47

36 *Andromedæ.*

36 Andromedæ is a close binary star, first measured by J. Herschel in 1830. The components are both pale yellow, and between the sixth and seventh magnitudes. The companion has only moved through an eighth of the circumference, while the distance increased from 0″·9 to 1″·5; but I thought it interesting to try an orbital determination even now. I made use of Herschel's method, obtaining the distances both by drawing tangents and by an interpolating curve.

Elements of 36 Andromedæ=Σ 73.

	°	′		
☊	57	54	e	0·6537
λ	142	19	P	349·10 years
γ	41	39	T	1798·80
			a	1″·54

γ *Leonis.*

γ Leonis was first measured by Herschel in 1782, when its motion was much faster than afterwards. The principal star is deep yellow, and between the second and third magnitude; the companion is pale yellow, and between the third and fourth magnitude. I found that the companion does not move in a straight line, so that its binary character is proved, and I calculated its orbit, though it has only moved through thirty degrees. The elements were derived in the same manner as those of 36 Andromedæ. I am at present correcting both orbits by equations of condition.

Elements of γ Leonis=Σ 1424.

	°	′		
☊ ……	111	50	e ……	0·7390
λ ……	194	22	P ……	402·62 years
γ ……	43	49	T ……	1741·11
			a ……	2″·00

σ Coronæ.

Herschel discovered in 1781 that σ Coronæ was double; and in 1802 he recognized its binary character. The components are of the sixth and seventh magnitude and both white, but the principal star is yellowish and the other bluish. The distance was in the commencement small, but afterwards it increased rapidly, thus rendering the observations much easier. The distance will continue to increase greatly. I employed Herschel's method in deriving a provisional orbit. When the construction was repeated all the observations were represented, except for one short interval; and I therefore prefered to correct the elements after Klinkerfues's second method. In this I encountered unexpected difficulties, and only obtained the orbit after repeated solutions of the equations and several trials. I must refer for the particulars of this calculation to a forthcoming work of Messrs. Wilson and Gledhill, where they have been given as an illustration of the application of this method. We have as yet seen but a small part of the ellipse described; but this part of it has, of course, been so much the more observed, and so much the nearer are the measures lying to each other. Much more uncertainty must, however, always prevail about these slow-moving systems than about those of quicker revolution, apart from other considerations, at least because the angle changes so little in one observer's lifetime, that systematic corrections cannot so easily be ascertained—that is, if they are not known otherwise. O. Struve's and Mädler's observations on σ Coronæ seem affected with large systematic errors. Mädler's angles are decidedly too large; his first angles, when the position was very oblique, are much too large; his later angles, when the position went through 180°, are about right, and then the correction changes its sign. His distances are also too large; but the deviation is likewise diminishing, and disappears at the end of the series.

Elements of σ Coronæ Borealis=Σ 2032.

	°	′		
☊ ……	18	21	e ……	0·7515
λ ……	71	36	P ……	845·86 years
γ ……	31	22	T ……	1826·93
			a ……	5″·885

α Geminorum.

α Geminorum is by far the finest double star in the northern sky. The components are between the second and third magnitude, and both nearly white. It was remarked and even observed very early, and subsequently it has been constantly measured, and its orbit has been often computed. In the first instance by Sir J. Herschel, perhaps prematurely, as he obtained a period of only two hundred and fifty years, while the true period must be nearer to a thousand years. The observations are, however, not quite so good as could be expected from so easy an object. The position-angles, in this instance as in many others, show rather regular deviations of a nature not to be explained by elliptic motion, the projected points being distributed in waves on both sides of the interpolating curve. As to the distance, although it is increasing, still most of the measures made by the same observer seem to diminish with the time; and it is not unlikely that this is the case also, though in a less degree, when taking the measures on the whole. I obtained the orbit by equations of condition on the foundation of Thiele's orbit.

Elements of α Geminorum = Σ 1110.

	° ′		
☊ ……	27 46	*e* ……	0·3292
λ ……	297 13	P ……	1001·21 years
γ ……	44 33	T ……	1749·75
		a ……	7″·43

The fundamental difficulty relating to binary stars arises evidently from the systematic deviations. It is easy enough to eliminate accidental errors and mistakes, but the method of least squares is not of the slightest use wherever the errors follow regular laws. Only too many observers, especially in years gone by, have entirely disregarded the existence of the latter; and it is yet reserved to the scientific critic to ascertain the corrections their measures do require, and whole series of labours, where this is not feasible, will be thrown away as useless. Nowhere is W. Struve's remark, "Errores personales majori observantis attentione possunt diminui," more out of place, for the systematic errors are very marked in the case of some of the most experienced observers who have worked under most enviable circumstances; and it thus appears at first glance that we cannot attribute these errors to neglect of some precaution or other in taking the measures. Whereas the beginner may consider it impossible that constant errors should be found in operations apparently so simple, the experienced investigator on looking back at the vast number of precautions he has had to devise to obviate the possibility of errors creeping into his results, may easily attribute the failure of others to neglect of one or other of these precautions; and very forced and trivial hypotheses not only may but actually have been started to explain the origin of systematic differences. W.

Struve observed artificial double stars, and thus convinced himself that his measures were free from constant errors. Bessel was the first to criticise this method. He urged the different circumstances under which the terrestrial and the celestial observations were made, and particularly the circumstance that while invariably only one artificial star is bisected at the time, on the contrary, it is generally necessary to look at both the stars in the sky simultaneously owing to imperfections in the driving-clock; and he found this surmise confirmed by the fact that while the difference between his and Struve's distances was large for average distances, it disappeared for very large or very small distances. The driving-clocks made in Munich were rather weak, and in particular is this the case with the large refractor in Pulkowa. W. Struve, however, spoke well of the clock that drove the refractor in Dorpat. After this the results derived from artificial fixed double stars had hardly any supporters except O. Struve, the follower of W. Struve. With intervals of about ten years this astronomer has examined his exceedingly large systematic deviations by aid of artificial objects near the horizon. And although his results do not perfectly agree with those of other investigators, even after the corrections, it must be admitted that we possess here a most valuable contribution to the study of the nature and change of these errors, and his measures are for this reason likely to be of lasting value. Dawes, the English observer, so famous for his accuracy, found also large errors in his measures. He thought to obviate them by placing the stars always vertically the one above the other by aid of a prism; but of course thereby only the influence of the angle of position is eliminated. O. Struve objected that the prism might injure the quality of the telescopic images and occasion errors of a different kind; but any optician will make a prism that will not impair the definition, and, moreover, the wires would undergo the same distortion as the centres of the stellar disks. I have proposed to use Dawes's prism on the sky for determining the influence of the position-angle for various distances, or to combine the use of it on artificial objects with W. Struve's original apparatus. This would be easier than to use O. Struve's apparatus. The observers in Cincinnati incline their heads so that the line joining the two stars is either parallel or normal to the horizontal section of their eyes, as Dawes proposed to do, though only in case the inclination of the head required was small. They derive corrections to be applied respectively to their measures in the two situations by a comparison between their results. It must be confessed that this is far more satisfactory than the method followed in Rugby, where the measures of a number of observers are treated all as one whole; but withal it is preferable not to have more than a couple of observers working with the same instrument. The following experiment is perhaps not unworthy of notice. With the refractor in Dunsink, Dr. Ball and I observed together the position-angles of the following binary stars (but perhaps the same drawback is attached to this experiment as to many of the attempts made to find out a constant difference through direct comparison—uncertain focussing):—

Epoch 1878·413. Power 300. Steadiness very good.

Name.	Ball. °	Doberck. °	B–D. °	Hour-angle. h	Definition.
ζ Boötis...	293·02	298·38	− 5·36	+0·7	Rather good.
Σ 1819 ...	14·30	19·12	− 4·82	+0·6	Very bad.
ε Boötis...	321·38	332·45	−11·07	+1·6	Very bad.
ξ Boötis...	283·08	279·03	+ 4·05	+1·7	Very bad.

The much-respected living double-star observer, Dembowski, is generally supposed to be entirely free from systematic errors; but with praiseworthy diligence he has nevertheless thought fit to subject this circumstance to a minute scrutiny. He has to this end made a list of the following 24 objects, where the motions are insensible or at least insignificant, which he is observing on an average 5 times each in each of the 24 hours star-time—a labour of hitherto unsurpassed extent, and which is likely to lead to most interesting results*.

Name.	R.A. 1875·0 h m	Dec. ° ′	Class.	Mag.
Σ 170	1 43·9	+75 36	III.	6·7, 7·8
Σ 191	1 52·1	73 15	IV.	6, 8·9
Σ 1169	7 57·2	79 52	VII.	7·8, 8
Σ 1321	9 6·0	53 14	VII.	7·8, 7·8
Σ 1350	9 23·9	67 21	V.	7, 7·8
Σ 1603	12 1·9	56 10	VII.	7, 7·8
Σ 1685	12 45·7	19 51	VI.	7, 7·8
Σ 2034	16 4·2	83 59	II.	7·8, 8
Σ 2326	18 17·5	81 27	VI.	7·8, 8·9
OΣ 353	18 22·6	71 16	I.	5, 7
OΣ 363	18 43·5	77 34	I.	7, 7
Σ 2452	18 57·8	74 37	IV.	6·7, 7·8
Σ 2571	19 35·3	78 0	V.	7·8, 8
Σ 2603	19 48·6	69 57	III.	4, 7·8
Σ 2675	20 13·1	77 20	IV.	4, 8
Σ 2796	21 18·0	78 4	VIII.	7·8, 9
Σ 2801	21 22·2	79 49	II.	7·8, 8
Σ 2806	21 27·0	70 1	VI.	3, 8
Σ 2893	22 10·6	72 41	VIII.	5·6, 7·8
Σ 2924	22 29·4	69 17	I.	7, 7·8
Σ 2923	22 29·7	69 44	V.	7, 9
OΣ 481	22 41·8	77 52	III.	7, 9
OΣ 489	23 3·9	74 43	II.	5, 7·8
Σ 3051	23 56·3	79 35	VII.	7·8, 9·10

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* For convenience' sake, and in order to avoid any bias from the results being known beforehand, the distances are expressed in Struve's classes: I. 0″–1″, II. 1″–2″, III. 2″–4″, IV. 4″–8″, V. 8″–12″, VI. 12″–16″, VII. 16″–24″, VIII. 24″–32″, IX. 32″–64″.

Struve and Dunér have promised to join Dembowski in his investigation, though they do not intend to carry it out so far as he. Winnecke and Hall will make similar observations, but it is not known to what extent. At Hall's proposal Struve has added the following six objects:—

Name.	R.A. 1875·0	Dec.	Class.	Mag.
	h m	° ′		
σ 244	6 37·7	+43 42	IX.	5·6, 8·9
σ 350	9 29·1	14 56	IX.	5·6, 8
Σ 1495	10 52·1	59 35	IX.	6, 8·9
σ 455	14 11·7	51 57	IX.	5, 7·8
σ 549	17 29·8	55 16	IX.	4·5, 4·5
σ 593	18 45·5	33 13	IX.	3, 6·7

It would hardly be advisable for very recent observers to join these mature men in their extensive research, because the systematic errors are changeable in the commencement. They cannot then be termed constant errors. But some objects might be measured with various magnifying-powers in different hour-angles. Christie has remarked that systematic errors might be ascertained by measuring the luminous rings with which the bright stars are surrounded; but perhaps objections might be raised because the respective phenomena are so dissimilar; but they might be determined by comparing the micrometric measures with the results of oblique transits, particularly if made chronographically. In particular, the systematic errors that affect the distances could, with sufficient approximation, be determined in this way, while the angles might be tried with the prism.

W. DOBERCK.

The Pulkowa Double-star Observations.—Part II.

WITH regard to the possible existence of constant differences between the measures made by him at different epochs, M. Struve finds, by comparing observations of double stars having no perceptible relative motion, that there is no trace of any such differences, either for position-angles or distances, since 1853; so that the corrections deduced from his observations of artificial double stars apply with the same force to all the measures made since that date. With regard to the observations made during the period 1839–1853, a superficial inspection of the results shows that the distances measured in the first three years are, with few exceptions, considerably greater than those of the years immediately following; on the other hand, the latter are smaller than those made after 1853; it also appears that the manner of observing position-angles changed about the year 1843. M. Struve then divides the measures made before 1853 into two periods, the first extending from the commencement of the measures in 1839 to 1843, the second from 1843 to 1853; and he compares the observations made during these two periods with the later measures to which his empirical formulæ directly apply. The results of this comparison lead to very important conclusions. It appears, first of all, that the measures of position-angles made during the first period 1839–1843 may be regarded as entirely free from errors of the nature of those which are expressed by the constant term of the formula (A). On the other hand, the comparison of the observations made from 1843–1853 shows that, for stars whose components are some distance apart, there is a perfect agreement between the position-angles measured during this period and during later years; whilst for close pairs there is reason to suppose that M. Struve has measured their position-angles in the same manner as before 1843, but there are not a sufficient number of observations of these close stars to enable him to be quite certain on this point. For the distances measured in these two periods, M. Struve finds that the constant correction $-0''\cdot081$ should be applied to the observations made from 1839–1843, and $+0''\cdot071$ to those made from 1843–1853, to reduce them to the standard of the measures made after the latter date. The existence of constant differences of considerable magnitude between the measures of the three periods is thus distinctly shown, but M. Struve can throw no light on the origin of these differences. Neither in 1843, nor about 1853, does he know of any thing having happened that could account for this phenomenon. With respect to the distances, it might perhaps be suspected that having remarked in 1841 that his own were in general greater than his father's, M. Struve might have been afterwards influenced by this knowledge. However, this suspicion loses its weight when it is considered that it does not apply to the change made at the same time in the measures of position-angles, nor to that made in the manner of observing distances about the year 1853. In fact, it was only during the course of the preparation of this Introduction that M. Struve undertook, for the first time, a direct comparison of the observations made in the three periods.

From a comparison of M. Struve's observations with those of his father, it appears that the position-angles measured by the latter have the same right to be considered free from constant errors as those of the former have for the period 1839–1843. With regard to the measures of distance, the very close pairs have been observed by W. Struve with almost perfect accuracy; but from distances of 2″ and upwards the measures are all too small. The error attains its maximum value of about a quarter of a second for distances of from 6″ to 8″. In this respect, also, there is a great resemblance between the manner of observing followed by both the Struves, as is very clearly seen by referring to the curves which M. Struve has laid down representing the absolute corrections to distances as observed by several astronomers. One of the most interesting of these curves is that representing the absolute corrections to distances of double stars as measured by Bessel with the Königsberg heliometer. Those readers of the 'Observatory' who are familiar with the history of Astronomy will remember that, about the year 1831, Bessel and W. Struve undertook in concert the observations of distances of double stars, the one with the Königsberg heliometer, the other with the Dorpat refractor, and that a comparison of the observations showed considerable differences in the measures. Some of these may be represented by the following table:—

Dist.	
″	″
2·89	Σ=B−0·199
5·60	−0·265
12·00	−0·169

By applying, however, to Σ the absolute corrections as determined by M. Otto Struve for the same distances, we obtain for distances measured by Bessel the absolute corrections:—

Dist.	
″	″
2·89	−0·055
5·60	−0·042
12·00	+0·017

It follows that, beginning from about 5″ distance, the greater distances have been measured by Bessel with almost absolute exactness. With regard to the negative corrections to the smaller distances, M. Struve believes them to be real, notwithstanding the indirect method by which they have been obtained; but he admits that the numerical values are subject to probable errors of considerable magnitude.

M. Struve also compares his measures of double stars with those of Dawes, Dembowski, Secchi, and Dunér. The following *résumé* gives the general result of the comparisons. Constant errors in the measurement of position-angles do not appear to exist in the observations of M. Dembowski; the measures of Dawes indicate the necessity of a correction in the same direction as those of M. Struve in his later observations; on the other hand, those of Secchi and, in a greater degree, those of M. Dunér show an opposite tendency. There are, in certain cases, indications of the existence of errors varying with the direction of the stars with respect to the vertical, but there are not sufficient materials furnished by published observations to enable one to pronounce with certainty on this point. M. Struve recommends to all observers of double stars who wish their measures to have the greatest possible degree of accuracy, either to undertake independent researches on artificial stars, or to follow the example of Baron Dembowski, who, for this purpose, proposes to measure certain double stars near the pole several times during each hour of their diurnal path.

From the comparisons of measures of distances, it appears that the distances of close pairs as measured by these observers are only subject to small constant errors. For distances of from 1″ to 2″ the measures of M. Dembowski are almost perfectly accurate, but those of Dawes require negative corrections, and those of Secchi positive corrections of considerable magnitude; for M. Dunér's measures the corrections, although insensible for very close pairs, soon become considerable and in the positive direction. From 2″ upwards, the positive corrections to the measures of M. Dembowski increase rapidly, and attain their maximum value at about the distance of 6″, thus presenting a remarkable agreement with the observations of W. Struve. The corrections of the distances measured by Dawes pass at the distance of 4″ from negative to positive, and increase gradually in the same direction. The distances measured by Secchi and M. Dunér agree in requiring a maximum positive correction at about 10″. The data are not sufficiently numerous to enable the corrections to measures of greater distances to be accurately determined; but it appears that in general they are measured by all the observers without constant errors of any considerable magnitude. Finally M. Struve recommends to all other astronomers whose measures of double stars have not been discussed by him on this occasion that they should connect their measures with the same system of corrections by carefully comparing them with one or other of the more extended series which have been the subjects of the preceding comparison.

In the observations of double stars contained in this volume, M. Struve has generally followed the methods pursued by his father; there are, however, a few differences in detail which it may be worth while to notice.

With the immediate object of collecting in a short space of time a certain number of measures of each star of his catalogue, W. Struve was generally content with making on the same occasion a single measure of double distance and two readings of position-angle of each pair of stars. It was only towards the end of his Dorpat observations, since the year 1835, that, being desirous of obtaining more exact positions for systems in rapid motion, he increased the number of measures made on each system. It will appear from the following details of M. O. Struve's method of observing, which may serve as useful hints to those engaged on similar work, that the only important difference between the observations of W. Struve and O. Struve consists in the fact that the determinations of each night have been rather more carefully made by the latter.

1. A complete observation ordinarily consisted of two measures of double distance and three readings of position-angle. If the measures did not agree well, or if the images were not good, M. Struve has frequently made one or two supplementary measures of one or other of the coordinates.

2. The measures of double stars have been made in general under good atmospheric conditions. When the images did not appear satisfactory for the higher magnifying-powers, M. Struve has preferred to turn to stars whose components are at a greater distance apart, which can be observed with lower powers, or else to cease observing altogether for the night.

3. The stars with rapid orbital motion, and in general the

"*lucidæ*" of the Dorpat Catalogue, have been observed by preference about an hour before or after sunset, the time of day ordinarily the most favourable at Pulkowa for steadiness of the images. On the other hand, the stars of the Pulkowa Catalogue, as the companions are frequently very faint, have been observed after dark.

4. Between each pair of successive measures of double distance, the point of coincidence of the micrometer-wires was changed about half a revolution of the micrometer-head; by this means M. Struve has endeavoured to make the observations perfectly independent. At the same time, if there were any eccentricity in the micrometer-screw its effect would be thus nearly destroyed.

5. In the observation of position-angles of double stars the components of which are within a distance of 32″, M. Struve has never employed the fine screw attached to the position-circle. To render the successive readings more independent of each other, he has always turned the micrometer-wires some distance away from the direction which is to be measured, sometimes on one side and sometimes on the other. On the other hand, in the observation of position-angles of stars exceeding 32″ in distance the fine screw has always been used; in this case M. Struve has always taken *four* readings of the position-angle, placing the wires parallel to the direction of the stars, twice by a motion in one direction, and twice by a motion in the opposite direction.

The effect of the greater care bestowed by M. O. Struve on the observations shows itself very distinctly in the considerable diminution of the probable errors of observations of distances compared with those of W. Struve; and, in a lesser degree, the same thing holds for the position-angles, although for these the balance in favour of M. O. Struve is very slight. In fact, for the latter's observations the exactness of the distances surpasses that of the position-angle, but for W. Struve's observations the position-angles are measured with the greater accuracy.

For W. Struve, Mean prob. error of an observation of distance = ±0″·150
" " " " position-angle = ±1°·14
For O. Struve, " " " distance = ±0″·102
" " " " position-angle = ±1°·13

In this volume there are occasionally to be found more detailed discussions on the relative motions of certain stars. M. Struve hopes that these will interest astronomers engaged in similar researches; but he is convinced that all the discussions, and the conclusions to which they lead, would have gained considerably in value if he could have availed himself of the researches and comparisons contained in the Introduction. But this was, of course, impossible, as the observations themselves have been used in these investigations. The distances measured have been reduced throughout the volume with the angular value of a revolution of the micrometer-screw first adopted (deduced from transits of stars) and not that given above; and the corrections for systematic errors have been computed from formulæ deduced by M. Struve in 1867, and not from his definitive formulæ (A) and (B); in fact, M. Struve states that his measures as published in this volume, like the mass of observations of double stars made by other astronomers, are only to be regarded as preliminary or approximate researches. However, any one wishing to do so can easily enough apply to the observed quantities the definitive corrections deduced by M. Struve from his observations of artificial double stars, and some other corrections, the necessity of which he points out.

As examples of M. Struve's method of treatment, I may refer to his discussion of the well-known systems *Castor* and 61 *Cygni*.

Castor.—The components are set down in order of magnitude, A=2·7, B=3·7, C=9·5. We have first the separate observations of distances and position-angles of A and B, with the corrected quantities in parallel columns; then come the mean values of the corrected quantities, taking about five separate observations together. From an inspection of these mean values, it appears that the measures of the last ten years indicate that about the year 1872 the distance betweeen the two stars was at its maximum, and that it now appears to be diminishing considerably. The time has then come when an attempt to calculate the orbit might be made with some chance of success, supposing that the effect of the third star, on account of the greater apparent distance, is insensible. It would be possible, at least, to examine to what degree the two principal stars, in their relative orbit, follow the laws of Kepler, and thus to arrive at general conclusions as to the relative mass and real distance of the third small star, the physical connexion of which with the principal system is proved by the almost perfect identity of its rather large proper motion with that of the brighter stars. M. Struve then gives the observations of the distances and position-angles of A and C, combining the measures of W. Struve and M. Dembowski with his own, and thus deduces the corresponding relations between the small star and the mean of the two brighter ones. These values, compared with those previously found by W. Struve, show that the change of distance has been considerably less, and that of position-angle greater than was to be expected from the determination of relative motion made by W. Struve. This is to be accounted for partly by the fact that W. Struve gave too great a weight to a single observation of his made in 1829, and it is partly owing to an extraordinary accumulation of accidental errors in M. O. Struve's measures from 1851–1853. On this account the calculation of the relative motion has been repeated with the result that the secular change in the position of the small star, with respect to the mean of the brighter stars, is in R.A. $=-3''\cdot1$, and in Dec. $=+0''\cdot3$, in place of $-1''\cdot3$ and $+3''\cdot8$ respectively, found by W. Struve. In the absence of all knowledge of the position of the common centre of gravity of the system, it is impossible to tell to what degree these changes ought to be attributed to orbital motion.

61 *Cygni*.—The magnitudes of the components are 5·3 and 5·9.

By making use of W. Struve's observations we have a series extending from the epoch 1830·68 to 1874·74. The mean values of the observed quantities are then reduced to differences of R.A. and of Dec.; from which it appears that between 1830 and 1852 the Δ R.A. increased 1″·43, but from 1852 to 1874 the increase has only been 0″·52. It is evident, then, that the supposition of a uniform rectilinear motion no longer satisfies the observation. This is more apparent when the data are treated by the method of least squares; it is then seen that the differences between the observed and calculated values of Δ R.A. are considerably greater than is consistent with the accuracy of the measures. These differences disappear almost entirely when a term is added to the equations involving the square of the time; so that, instead of a uniform rectilinear motion, as the comparison of measures made from 1830–1852 with former observations seemed to indicate, there is good reason to believe in the existence of an orbital movement.

But it would take up too much space were I to draw attention to more of the conclusions to which M. Struve is led from a discussion of his observations; of course those who are interested in the subject will refer to the book itself. I would merely remind such that the corrected distances and position-angles given in the volume require to be still further corrected to bring them, in M. Struve's opinion, to the greatest attainable degree of accuracy.

We have, then, in this volume, observations extending over a period of 37 years, which cannot fail to be of great interest to those who are engaged in similar researches, the greater as the measures extend through such a number of years. In such a case as this there is, no doubt, an advantage in deferred publication if we can be sure that this does not mean total suppression, but there is always more or less danger of this catastrophe. Let us hope that M. Struve will be able shortly to publish his next volume, which he expects will give him an opportunity of resuming the discussion of the relative motions of certain stars, and of increasing the importance of his researches by comparing the relative motions with the proper motions deduced from meridian observations made at Dorpat and at Pulkowa. I need not say that all English astronomers will gladly welcome the appearance of Volume x. of the *Observations de Poulkova.*

A. W. Downing.

CORRESPONDENCE.

To the Editor of 'The Observatory.'

The Variable Star T *Coronæ, and Prof. Newcomb's 'Popular Astronomy.'*

Sir,—

A passage in Prof. Newcomb's recently-published 'Popular Astronomy,' respecting the discovery of the remarkable variability of the star T Coronæ in the year 1866, recalls to mind the controversy which arose as to the time of that discovery, and by implication the degree of rapidity with which the extraordinary increase of brightness took place. The first person in Europe who saw the star with the naked eye appears undoubtedly to have been Mr. Birmingham, at Millbrook, near Tuam, Ireland, who noticed it on the night of May 12, 1866, between $11^h\ 30^m$ and $11^h\ 45^m$, Tuam M.T. (between $12^h\ 0^m$ and $12^h\ 15^m$ G.M.T.), and considered it to be "fully equal to α Coronæ in size, and superior to it in brightness"; in another account, "very brilliant, of about the 2nd magnitude"*. Later on the same night it was independently noticed by Mr. Farquhar, of the Patent Office at Washington†, of which the longitude is about 5^h west of Greenwich; and on the following night (May 13) by several observers, particularly by Dr. Julius Schmidt, Director of the Observatory at Athens‡, and by M. Courbebaisse, an engineer at Rochefort§. The latter remarked that he was watching the constellation on May 11 and saw nothing then of the stranger. But Dr. Schmidt went further and was positive that no star was conspicuously visible there to the naked eye on previous nights, including the preceding night, May 12, up to $9^h\ 45^m$ (Athens time, corresponding to about $8^h\ 10^m$ at Greenwich), when he was particularly observing the constellation, seeking for the variable R Coronæ; whilst on May 13, as soon as the sky was sufficiently dark, and even before the twilight had quite gone, he was immediately struck by the appearance of the new star, which completely altered the familiar shape of the constellation.

It is scarcely necessary now to refer to the claim of Mr. Barker, of London, Canada West, after the discovery was published, to have seen the star more than a week before, as early, indeed, as May 4; for this has been disproved by Mr. Stone's letter in the 'Monthly Notices,' Vol. xxvii. p. 57. But some time afterwards, Mr. Walter, Surgeon of the 4th Regiment, stationed in North India, wrote to Mr. Stone ‖, and stated that he had seen the star in question at about 8 o'clock on the evening of May 12; which time, as the longitude of his station was 78° 33′ east, would correspond to about $2^h\ 45^m$ at Greenwich, and $4^h\ 15^m$ at Athens. Mr. Walter described the star as quite as large and bright as (if not rather more so than) α Coronæ. If he has not made an error of a day in recording the date of his observation, he was probably the first to notice the star; and the star, contrary to Dr. Schmidt's negative evidence, must have been visible in Europe as soon as it was dark on the evening of May 12. But Mr. Walter's account was written so long after the observation that I cannot but think it likely that his observation was made on May 13 instead of 12.

Such is a statement of the facts as far as they are known; and we will now refer to Prof. Newcomb's history of the matter, which is erroneous in two particulars. He says (p. 432), "On the 11th and 12th of that month" (*i. e.* May) "it was remarked independently by at least five observers in Europe and America, one of the first being Mr. Farquhar, of the United States Patent Office." Now, on reference to the 'Astronomische Nachrichten' in the place before mentioned*, it will be seen that Mr. Farquhar's observation was on May 12, so that if any one saw it on May 11 (of which I can find no satisfactory evidence), Mr. Farquhar could not have been "one of the first." Further on Prof. Newcomb says, "Schmidt, of Athens, asserts in the most positive manner that the star was not there on May 10th, because he was then scanning that part of the heavens, and would certainly have noticed it." I presume there is a printer's error here, for Prof. Newcomb can hardly have overlooked the importance of the date, and that Dr. Schmidt twice insisted upon it (the second time in a letter to myself dated October 20, 1866, and printed in the 'Astronomische Nachrichten,' No. 1615, Vol. lxviii. p. 105), that the star was not visible to the naked eye up to at least 10 o'clock on the 12th of May.

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* Monthly Notices, Vol. xxvi. pp. 310, 275.

† Astronomische Nachrichten, Vol. lxvii. p. 201.

‡ *Ibid.* p. 87. § *Ibid.* p. 31.

‖ 'Monthly Notices,' Vol. xxvii. p. 316.

I do not know whether you will think the above remarks worth a place in the widely-diffused 'Observatory'; but it appears to me that Prof. Newcomb's book is so excellent and useful that it is desirable that such errors as those I have alluded to should be made generally known. The question, too, of the suddenness of the outburst of T Coronæ is an interesting one.

I am, Sir, yours faithfully,

Blackheath, 1878, Sept. 23. W. T. LYNN.

Relative Brightness of Venus and Mercury.

SIR,—

Having been impressed on many occasions (while observing Venus and Mercury during daylight) with the great relative brightness of Venus as compared with Mercury, I availed myself of the very favourable conditions which their close conjunction this day afforded me to make a careful comparison of the relative brightness of the two planets when they were in the field of the telescope together. It is difficult to assign any very exact ratio of the brightness of two such objects; but certainly the conditions for the attempt were peculiarly favourable. So far as my judgment serves on such a subject, the result of my observation is that I feel I should not be far from correct if I stated that I consider the brightness of Venus to be quite twice as great as that of Mercury, or somewhat like the difference in that respect between clean silver and clean fresh-cut lead.

The relative deficiency of the brightness of Mercury is the more remarkable, as, considering the much greater nearness of Mercury to the Sun, his brightness *ought* to be fully three times greater than that of Venus, whereas it appears to me to be quite the contrary, thus indicating some very special condition of the surface of Mercury or of his envelope in so remarkably modifying the action of the sunshine on his surface as to reduce in so great a degree the brightness of the light he reflects. I trust the very favourable conditions which fortunately attended this remarkable conjunction will have led many who are possessed of suitable instruments to observe it; and I hope the subject of this note has attracted the notice of several observers, and that the question may receive the careful attention it appears to me to deserve. The telescope I employed on this occasion was a fine equatoreal refractor by Cooke, of York, 8 inches aperture. The powers used were 80 and 120. The sky perfectly clear, and the air in fine condition for sharp definition.

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* Vol. lxvii. p. 201.

I am, yours very truly,
JAMES NASMYTH.

Penshurst, Kent,
Sept. 26, 1878.

Conjunction of Mercury and Venus.

SIR,—

On Sept. 25, at about 17^h, when twilight had become too strong for further meteor-observations, Mercury and Venus, about 6° or 7° above the E. horizon, and distant less than 20′ from each other, formed a rare spectacle. I saw them first at $16^h\ 28^m$, and they continued in view of the naked eye until $17^h\ 40^m$, when Mercury was lost in the increasing daylight. Both planets scintillated strongly, notably Mercury, whose light exhibited very quick fluctuations. In point of brightness he must have exceeded the 1st mag.; for I made comparisons with α Leonis (about 15° W.), and found that star dsiappeared at $17^h\ 31^m$, or 9^m before the planet, though the latter was far more deeply immersed in the twilight.

The night was extremely clear, and after 15^h the zodiacal light became very striking amongst the stars of Leo and Cancer. We are just arriving at the best season for A.M. observations of this phenomenon. Last October it was seen here very plainly on every fine morning.

Yours faithfully,
W. F. DENNING.

Ashleydown, Bristol.
Sept. 26.

*Observations of Jupiter at Adelaide**.

SIR,—

By to-day's mail I send the following for the R.A.S., viz.:—Observations of Transits, Occultations, and Eclipses of Jupiter's Satellites in 1877; Total Eclipse of the Moon, 1877, Feb. 28; Partial Eclipse of the Sun, 1878, Feb. 2; Transit of Mercury, 1878, May 6. A few sets of east and west parallax observations of Mars at opposition were taken, which I must send you in a future letter. The same sets comprise a large number of differential measurements of R.A.; but my chronograph was not very good, and the instrument required a better clamp in R.A. The observations may, however, possess some value, and I will therefore send them for what they are worth. With a better R.A. clamp, which I will make, and a new chronograph I have made since, I think I shall be able to secure better results in future.

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* Communicated by the Astronomer Royal.

With regard to Jupiter, the visibility of the satellites through the edge of the planet has been very carefully looked for; and on two occasions I felt quite certain I saw the satellite at occultation within the disk, viz. on June 19 at occultation disappearance of Sat. II., and on July 2 at disappearance of Sat. I. Mr. Ringwood also saw the same phenomenon at disappearance of Sat. II. on August 29. I thought I could see Sat. III. through the limb at reappearance on June 9, and Sat. II. at disappearance July 21, but could not be certain. In every other case the occultation was perfect at the limb. On June 19 Sat. II. was visible through the white belt south of the equator, and on July 2 Sat. I. was seen for the space of one minute through the southern dark belt*. It would be interesting to know whether this has been seen by other observers. The definition on each occasion was very good, or I should think I might be deceived. I may mention that the other evening I was surprised to see Sat. IV. in transit appear as a dark spot instead of the usual bright point when on the less illumined surface near the edge of the planet; in fact it was at entry nearly as black as its shadow, which was just passing off on the other side. This I know has been seen before, both with Satellites III. and IV., and must, I presume, be attributed to one side of the satellite reflecting less light than the other, or else to some violent changes in its atmosphere. Before it came on to the disk I noticed that it was fainter than usual.

It may be worth recording, perhaps, that in 1877 the dark belts of Jupiter had not, to my eye at least, the same bright metallic lustre that they had in 1876, but appeared to be covered with a thin haze and looked foggy; and this year the southern dark belt is seldom so highly coloured or so well-defined as the northern one.

Adelaide Observatory, I am, Sir, yours faithfully,
1878, August 8. C. TODD.

[Mr. Todd's observations include two eclipses of Sat. IV., in the first of which (1877, Aug. 29) the satellite never disappeared, but was reduced to a mere speck of light skirting the margin of the shadow—a very important observation.—ED.]

The Moon.

SIR,—

In the 'Observatory' for October 1878, No. 18, p. 192, the following passage occurs:—"On Aug. 17 I made a comparison with a star 9–10 mag.; but unfortunately the latter is not to be found in any catalogue. *What a gigantic task lies before astronomers—to catalogue all stars down to the ninth magnitude! How useful it would be, and how pleasant to find a catalogued star on making comparisons!*" And in a note on page 169, the first in No. 18, "Double stars are now-a-days observed in over twenty large observatories. Small series of measures taken by inexpert amateurs would therefore seem to be of minor importance, so much more as want of system prevents thorough examination of a small number of results. In some such cases it might even be difficult to ascertain whether the measures were made *bonâ fide*."

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* [During this time it would have moved through a space of about $0''\cdot3$ within the limb.—ED.]

May I ask, Mr. Editor, permission to offer a remark or two in connexion with these passages in reference to the Moon. It would appear that the mapping of the Moon has attained a degree of development which it is difficult to conceive can be surpassed. There can be no doubt that many astronomers will consider that the magnificent production of Schmidt leaves nothing to be desired! We believe that as far back as 1863 an important desideratum in Selenography was suggested, namely, "A Catalogue of Lunar Objects." In the words of the first passage above quoted, we may say, "What a gigantic task lies before selenographers—to catalogue all the craters on the Moon's surface, from the magnificent *Copernicus* to the most minute craterlet that can be discerned with the largest lens or mirror." "You may as well attempt to catalogue the pebbles on the sea-shore," said a well-known astronomer shortly after the above suggestion had been acted upon and a catalogue had been commenced. Although this catalogue was commenced on a systematic plan, but small progress has been made; for out of the 32856 craters given by Schmidt not more than 2000 objects, including mountains, have been entered in the Registry provided for the purpose. On comparing a few of these published objects with Schmidt's map it has been found that *some are not in it*, a fact calling for a most careful examination of Schmidt's map, not for the purpose of depreciating the greatest selenographical work that has yet appeared, but for the real advancement of Selenography; and as the work proceeds how pleasant will it be to find that the smaller registered objects *become well known* to the careful and persevering observer, so much so that a new formation may be easily detected! And this leads to a remark on the observations of mere amateurs. A good opportunity, by the publication of Schmidt's map, is now afforded for taking up the study of Selenography *in earnest*; sketches of isolated regions and catalogues of the objects seen and numbered, without reference to work previously effected, oftentimes by inexpert amateurs, will now become of minor importance; the work must be taken up thoroughly, systematically, and energetically, Schmidt's map being made the basis of operations. The examination of each district should be undertaken by at least three observers; a tracing from the map of the district chosen should be furnished to them, and a well-known Selenographer appointed to examine the observations. If twelve observers were to undertake four neighbouring districts, one of each party of three employing telescopes of *large* aperture, much good work might be effected towards extending the number of catalogued objects now before the public. Gentlemen who may be inclined to take part in this work will obtain every information by applying to Herbert Sadler, Esq., Honorary Secretary of the Selenographical Society, 32 Manor Street, Clapham, S.W., a society having for its object the advancement of our knowledge of the Moon's surface, and the changes it may undergo, so far as observation may assist us in detecting them.

I am, Sir, yours faithfully,
W. R. BIRT.

Stratford,
1878, October 8.

γ_1, γ_2, and γ_3 *Andromedæ.*

SIR,—

In the "List of Double Stars for October 1878," given in the 'Observatory' for that month, p. 186, the colours of the treble star γ Andromedæ are stated to be golden, green, and blue.

I can, however, positively state, from actual observation, that the colour of the second is not green, but yellow; and I shall be obliged by your inserting this letter to that effect.

On the 5th February, 1869, I was fortunate enough to have an exceptionally excellent view of the object, which I recorded in the 'Monthly Notices,' Vol. xxix. No. 5, p. 195, by reference to which it will be found that I then saw the three stars distinctly, with their colours, and that the second was light yellow.

From the close proximity of γ_2 and γ_3, γ_2 may easily be thought to be green from the colours of γ_2 and γ_3, which are yellow and blue, being smudged together, as if mixed on a pallet, and producing green; but when γ_2 and γ_3 are separated as I saw them on the day alluded to, with well-defined circular disks and clear spaces around them, no such confusion need arise.

I have often seen the two stars, γ_2 and γ_3, apparently partly green when indistinctly separated; but, if seen as on my view at the time named, there could be no mistake in the matter.

The instrument used was my 9-inch equatoreal by Cooke and Sons (which formerly belonged to the late Captain Jacob), with a power of 720.

Having compiled a list of popular celestial objects for private use, in which I have described the colours, as above stated, I am anxious to support its correctness in so interesting an object. The colours are golden or deep orange, light yellow, and blue.

I am, Sir, yours truly,
THOMAS BARNEBY.

Morton House Observatory, near Worcester,
1878, October 7.

Naked-eye View of Jupiter's Satellites.

SIR,—

On the evening of the 12th inst., as Mrs. McCance (my mother) and I were approaching home, we remarked Jupiter shining in the south. After a moment Mrs. McCance observed, "He looks double, as if there was a small star quite close to him, to the left of and a little above him." This was about 5.55 P.M.

On examining Jupiter in the telescope about ten minutes afterwards I found that there was no conspicuous star in his neighbourhood, but that satellites II., III., and IV. were all to the east of the planet, and pretty close to one another; IV. was very much duller than either of the others.

After a few questions I was satisfied that the satellites really were seen with the naked eye. Mrs. McCance described the appearance as varying in visibility, sometimes looking like a large ray from the planet, but generally like a distinct and fairly visible little star. She could still see this at 6.30 P.M.

Through the glasses I use for my short sight I sometimes thought I could see something of the phenomenon; but I fear this was only fancy.

Mr. Webb ('Celestial Objects,' p. 144) mentions some naked-eye observations of the satellites being made "in twilight, before the glare of the planet came out;" and on the evening in question there was at first a fair amount of twilight, the Sun not having set an hour; so very likely such time is the best for these observations.

I do not know whether this account may have any interest; but I send it you on the chance.

I remain, Sir,
Yours faithfully,
JAMES L. McCANCE.

Bathfern, Rayners Road,
Putney Hill, S.W.,
1878, October 17.

NOTES.

THE SATELLITES OF MARS.—In an able memoir, published by the Washington Observatory, Prof. Asaph Hall has discussed all the observations of the two satellites, and has deduced corrected elements of the orbits, from which tolerably accurate ephemerides may be prepared for the next opposition in 1879, November. Prof. Hall commences by giving an account of his discovery, from which it appears that it was the result of a systematic search undertaken with that express object. Only two astronomers, Sir W. Herschel and D'Arrest, had previously undertaken the task; but their instrumental means were far inferior to those which Prof. Hall had at his command, and he was thus encouraged to proceed. The first observation of *Deimos* (the outer satellite) was made on August 11, and of *Phobos* (the inner) on August 17; and from these dates Prof. Hall observed them assiduously, the former till October 31, and the latter till October 15, several times on each night as a rule. The observations thus obtained at Washington (52 of Deimos and 43 of Phobos) form by far the most complete series extant; and to Prof. Hall is due the credit, not only of having discovered the satellites, but also of having observed them so perseveringly that their orbits can be determined with great precision. From this splendid series of observations Prof. Hall deduces the following elements by first computing approximate circular elements, and then correcting these by equations of condition :—

	Deimos.	*Phobos.*
Epoch......	1877, Aug. 28·0 G.M.T.	1877, Aug. 28·0 G.M.T.
Period......	$1^d\cdot262429$	$0^d\cdot3189244$
	$=30^h\ 17^m\ 53^s\cdot86 \pm 0^s\cdot985$	$=7^h\ 39^m\ 15^s\cdot07 \pm 1^s\cdot123$
Log. μ......	2·4550955	3·0526147
a	$32''\cdot3541 \pm 0''\cdot0118$	$12''\cdot9531 \pm 0''\cdot0142$
ι	$35^\circ\ 38'\cdot7$	$36^\circ\ 47'\cdot1$
☊	48 5·7	47 13·2
ϖ............	$40\ \ 53\cdot6 \pm 5^\circ\ 5'$	$45\ \ 30\cdot4 \pm 2^\circ\ 11'$
e	$0\cdot00574 \pm 0\cdot00049$	$0\cdot03208 \pm 0\cdot00141$
u	$357^\circ\ 30'\cdot5$	$285^\circ\ 20'\cdot2$

The mean distance (a) is that at distance unity; the inclination (ι) and longitude of node (☊) are referred to the equator for 1877, August 28; u is the argument of latitude or angular distance from the node at the epoch.

The measures were made by bisecting the apparent disk of Mars with a single wire, and therefore required a correction for the figure of the disk as well as for differential refraction. The above elements were deduced from 98 equations of condition for Deimos, and from 79 for Phobos; and the residuals show that the probable error of a single observation is $\pm 0''\cdot4$, the error of position-angle being expressed in seconds of arc of a great circle. The largest residuals are $+2''\cdot11$ and $+2''\cdot41$ for Deimos in dist. and pos.-angle respectively on August 17; but though the observations were made in morning twilight, and somewhat hurriedly, Prof. Hall has preferred to let them stand. He adds, "All the observations were weighted before the comparisons, and no observation has been rejected. The method of rejecting observations after the comparison by means of criterions which are based on the residuals and the probable errors, seems to me to rest on a false logic, and to lead to the dangerous practice of trimming observations for the purpose of procuring an apparently accurate result, which, in fact, may be wide of the truth."

The planes of the orbits of both satellites are very nearly coincident with the equator of Mars; and for Deimos the excentricity is so small that circular elements may be considered as sufficient for the observations.

The resulting mass of Mars is:—from Deimos $\frac{1}{3095313 \pm 3485}$, from Phobos $\frac{1}{3078456 \pm 10104}$; the mean of which, by weight, is $\frac{1}{3093500 \pm 3295}$.

Prof. Hall has compared the Washington elements with the more or less complete series of measures made at various observatories From observations of the two satellites at Cambridge (U.S.), Glasgow (U.S.), and Pulkowa, the resulting mass of Mars is $\frac{1}{3107713}$, the values ranging from $\frac{1}{3023319}$ to $\frac{1}{3400630}$. These determinations show that probably there is no important constant error in the Washington measures; and considering the great advantages of the 26-inch refractor in such observations, Prof. Hall adopts the value given by the Washington observations alone, viz., $m = \frac{1}{3093500 \pm 3295}$.

The peculiarities of the areocentric motion will be readily deduced from the elements. Phobos has an hourly motion of 47°·033, and will therefore rise in the west and set in the east, meeting and passing Deimos, whose hourly motion is only 11°·882. Deimos is distant 14,500 miles from the centre of Mars, and Phobos 5800 miles from the centre, or 3700 only from the surface, so that the horizontal parallax amounts to 21°; and this large value will produce apparent excentricities in its motion, and cause it to appear as a variable star.

With regard to the brightness, Prof. Hall estimates the outer satellite to be of the 12th magnitude, and the inner 11·5, when free from the glare of the planet. A table of the comparative brightness at favourable oppositions since 1783 serves to explain the circumstance that these satellites were not discovered before. The only oppositions at which the brightness equalled that of 1878 (1·35) were in 1798 and 1845, and the planet had then a larger southern declination (14°·3 S. in 1798, and 19°·4 S. in 1845, as against 11°·9 S. in 1878). At the next opposition, in 1879, the brightness will be little more than half (0·73); but, on the other hand, the declination will be 18°·3 N., an important advantage. The brightness 1 corresponds to 1877, Oct. 1, when the outer satellite was seen with a 9½-inch equatoreal at Washington. On October 31, when it was last observed with the 26-inch refractor, the brightness was 0·52. Thus there is a good prospect of securing a fair series of observations in 1879, and Prof. Hall has computed data for an ephemeris. The probable errors in the areocentric longitude resulting from the probable errors of the periodic times will then be:—for Deimos ±2°·6, for Phobos ±36°·5; so that even for the latter there will be no uncertainty as to the number of revolutions performed in the interval. We are sure that our readers will join with us in congratulating Prof. Hall on the completeness of his work.

DISCOVERY OF DARK LINES IN THE SOLAR SPECTRUM CORRESPONDING TO OXYGEN*.—From a series of very careful measurements of photographs of the spectrum of the Sun and of Oxygen obtained with diffraction-gratings, compared with the results of other observers, Prof. J. C. Draper is led to the conclusion that there are 65 lines in the solar spectrum not appropriated to any other element, which may be assigned with great probability to oxygen. The wave-lengths being expressed to the hundredth of a tenth-metre, out of the 65 lines the coincidence is absolute in 17; in 4 the difference is only five hundredths; in 22, ten hundredths; in 4, fifteen hundredths; in 11, twenty-one hundredths; and in the remaining 7 the greatest difference is thirty-five hundredths, or about that which Ångström has made in different measurements of the same line in the solar spectrum. The wave-lengths of the lines in the solar spectrum were determined by projecting the photographs on a scale of wave-lengths, in which a tenth-metre was represented by 5 millimetres, by means of a specially contrived lantern. The apparatus was so adjusted that the leading lines of the photograph as projected corresponded with those of Ångström or Cornu (in the ultra-violet), laid down beforehand on the scale; and thus the errors in Ångström's chart were corrected. From the measures thus obtained a chart of the spectrum was constructed, which extended from E in the green to P in the ultra-violet. For the lines of oxygen a spectroscope with two flint-glass prisms of 60° was used, and with this photographs of the spectrum given by the condensed electric spark between electrodes of platinum and iron in atmospheres of oxygen and of oxygen and nitrogen were taken. These photographs were projected on the scale, and the oxygen and iron lines measured. The wave-lengths of the oxygen lines from 3864 to 4415 were based on forty-seven iron lines, the wave-lengths of which were inferred from the corresponding lines in the chart of the solar spectrum. From 4415 to 4705 the iron lines did not photograph, and the portion of the curve of wave-lengths was therefore constructed from the wave-lengths of oxygen and air-lines already given by various authorities. Every precaution was taken to have the oxygen as pure as possible, and photographs of the spark between points of the purest platinum were also taken for comparison with those between points of iron and platinum for the detection of any error arising from impurity in the iron.

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* Amer. Journ. 1878, October.

The oxygen lines in the solar spectrum appear to be very faint when no other element furnishes a line falling on the same wave-length. This is shown by a comparison of the photographic strength of the lines in each spectrum, and also by a chart of a portion of the solar spectrum about 4070. The oxygen lines represented in this diagram are among the strongest in the electric spectrum of oxygen; yet the equivalent lines in the solar spectrum are faint when compared with Ca and Fe. This would seem to indicate a low absorbing power in the gaseous non-metallic elements as compared with the same power in the case of metals in the vaporous state, and would explain why many of the lines in the solar spectrum which represent oxygen have been overlooked. It is to be remarked that Prof. J. C. Draper's table presents what may be called the oxygen region of the spectrum, only a few oxygen lines lying outside its limits; and this happens to be the region in which the photographic apparatus and chemicals were most sensitive.

In conclusion, Prof. J. C. Draper offers "the suggestion that the proof of the presence in the solar envelopes of oxygen, and other substances giving faint lines, is a problem not to be solved by the comparison of two spectra of small dispersion. The solar spectrum in certain parts is so crowded with lines presenting all kinds of details that the only satisfactory way is to make measures of the positions of these lines on a large scale and as truly as possible, and then compare with these the most accurate measures of oxygen lines that can be made."

THE ECLIPSE OF THE SUN, 1878, JULY 29*.—In the 'New York Times' of August 16, Prof. C. A. Young gives a summary of the results of the recent eclipse:—"As regards the physics of the Sun and the corona the principal result is to demonstrate a decided sympathy and connexion between the condition of the Sun's visible surface, as indicated by the number and character of the sun-spots, and the constitution of the corona." He considers that the question whether, like the sun-spots and chromosphere, the corona also would show a difference of condition from that indicated in 1869 and the later eclipses, "has received an emphatic and affirmative answer." As to the brightness of the corona there is considerable difference of opinion; but Prof. Young, in common with the large majority of those who also saw the eclipse of 1869, thinks that the corona on that occasion, though perhaps less extensive, was many times more brilliant than in the late eclipse, whilst in 1870 its brightness was intermediate. "While, however, there may be room to question the conclusion that the corona this year was uncommonly faint, there can be no question that its spectrum was profoundly modified." The bright lines so conspicuous in former eclipses were this year so faint as to be seen by only a few observers; and this was especially remarkable in the case of the 1474 line, for, though many observers saw it plainly just at the beginning and end of totality, nearly all entirely lost sight of it during the middle of the eclipse. The slitless spectroscopes showed "only a disappointing continuous band of colour," though in 1871 Mr. Lockyer with a similar instrument saw four coloured images of the corona. One or two observations of some interest in their relation to previous work were made. Prof. Rockwood saw a bright red line in the chromosphere spectrum very near B, presumably 534 K, a line exceedingly difficult to see under ordinary circumstances. This explains an observation by Mr. Pogson in 1868. Both H

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[*] Amer. Journ. 1878, September.

lines (calcium) were also seen bright by Professors Young and Rockwood, confirming an observation made by the former six years ago at Sherman. The reversal of the Fraunhofer lines was well observed at the beginning of totality, and Janssen's observations in 1871 of the dark Fraunhofer lines in the continuous spectrum of the corona was also confirmed by several observers, "thus showing that a considerable percentage of the coronal radiance is mere reflected sunlight," though the faintness of these dark lines shows equally clearly, in Prof. Young's opinion, "that the particles which reflected the sunlight are themselves also self-luminous, as, of course, they ought to be so near the Sun."

In 1869, 1870, and 1871, the gaseous elements of the corona (the hydrogen and 1474 stuff) were in such quantity and condition, and rose so high above the solar surface, that their lines were conspicuous in the coronal spectrum. "At present the condition is reversed. The gases are either too small in quantity or too cool to be conspicuous. The lesson, and it is an important one, is that to a certain extent the corona sympathizes with the sun-spots.

"It certainly looks probable also that while the gaseous elements of the corona are strictly solar, the non-gaseous matter (the coronal dust or haze) is of extraneous and very likely meteoric origin. At any rate the extent of the corona was certainly not less than on former occasions, whatever may have been the case with its brightness. In fact it has never been traced quite so far from the Sun before as this time by Langley and Newcomb, who followed it out for 6° along the ecliptic, a success partly of course due to the clearness of the air at their elevated stations. Now this is quite consistent with the theory that meteor-streams furnish the hazy matter of the coronal envelope, since, so far as we can judge, they have nothing to do with sun-spots."

Prof. Young sums up thus:—"The eclipse of 1878 has added a new planet to the system, and has demonstrated that the unknown cause, whatever it may be, which produces the periodical sun-spots at intervals of about eleven years, also affects the coronal atmosphere of the Sun."

While by no means concurring with Mr. Lockyer that Mr. Meldrum's investigations upon Indian cyclones have already demonstrated connexion between sun-spots and the weather, Prof. Young thinks that "the result of the eclipse goes to show such a periodical change in the state of the solar atmosphere as might very possibly produce a sensible effect upon the earth; whether it does so or not is a question which can be settled only by a careful and systematic investigation of the facts."

THE CLIMATE OF CORDOBA AND THE SUN-SPOT PERIOD*.—In an account of his work at Cordoba, Dr. Gould remarks that the climate, which during the first years of his sojourn was peculiarly favourable for astronomical work, has shown itself variable to a remarkable degree. Although observations are made at every opportunity, even through haze or between the clouds, when lightning is flashing over a part of the sky, yet in the last year no observations were possible on more than half the nights in March and April, and only ten in these two months were clear; whilst in June and July the case was even worse, there being only seven clear nights. This year the weather has been just as bad, only one night in the seventy-nine already elapsed having been free from cloud. "The contrast between such condition of the weather and that which prevailed some years ago is very striking, and seems to point to some periodic fluctuation." A like periodicity has come to light from the study of the climate of Buenos Aires, as mentioned in Dr. Gould's annual report for 1876, just printed.

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* Amer. Journ., 1878, June.

With reference to this point, Dr. Gould remarks:—"In the first place, the difficulties which have attended previous attempts to determine the relation between the mean temperature and the number of Sun-spots have been found to disappear when the local influences which affect the temperature are removed. I have not at hand the means of knowing what these influences may be in other places, but the thermic wind-rose has sufficed for Buenos Aires. Determining for each year the mean direction of the wind, and deducting its effects from the mean temperature as observed in the same year, there results a curve which follows that of the Sun-spot so closely as scarcely to leave any thing more to be desired. I have only used the mean of the wind-roses determined for ten days in each spring, summer, autumn, and winter; nor has the mean velocity of the wind been brought into the computation. More thorough calculations based on a wind-rose more minutely determined, and upon the consideration of the total amount of air of a different temperature flowing in, would doubtless increase the degree of agreement; yet the discordance between the mean temperature observed and that which results from the empirical calculation does not amount in any one of the twenty years, 1856–1875, to so much as might fairly be attributed to errors of observation."

Denoting Wolf's relative number of Sun-spots by r, the formula for the mean temperature of Buenos Aires would be $17^{\circ}{\cdot}50 - 0^{\circ}{\cdot}00727\,r$ in degrees centigrade. After deducting the effect of the wind, the mean temperatures were by observation 16°·47 in 1870, and 17°·51 in 1875, and by calculation 16°·63 and 17°·49 respectively. The difference 0°·16 in 1870 between the formula and observation is by far the greatest in the series, the mean discordance being only 0°·056. Dr. Gould adds:—"It is manifest that if the variations of the terrestrial temperatures follow those of the Sun-spots, and are thus adequate to account for the correspondence observed between these and the variations of the magnetic declination, all necessity for assuming any direct and transcendental connexion between this latter and the disturbance of the Solar surface disappears."

The mean annual direction of the wind varies through an arc of 60° during the twenty years in question, having been 70° in 1857, 77° in 1870, and 130° in 1874, reckoned from north through east. The correspondence between its variations and those of the spots found at Oxford by Mr. Main does not hold at Buenos Aires; but the component perpendicular to the meridian shows two periodic terms of 2·78 and 3·70 years respectively, being the quarter and third of 11·11 years, the mean length of the Sun-spot period. Another and yet more marked cycle shows itself in the frequency of storms. Whether the annual number of winds of force five, six, seven, or upwards, or the mean annual force be considered, a periodic fluctuation manifests itself, having its maximum early in 1864, the minimum between 1871½ and 1876, and the period between eighteen and twenty-three years, which latter cannot yet be fixed with accuracy, being so nearly equal to the period comprised in the series of observations.

Dr. Gould announces, with regard to his astronomical work, that the Atlas of the Uranometry, consisting of thirteen charts, on the scale of a globe of one metre radius, and containing all stars to the 7·0 magnitude inclusive within a radius of 100° around the South Pole, is completed, and some advance copies have been received.

It is worth mention that the observatory clock has been twice stopped during the last year by earthquake-waves (although the tremors were not felt by persons in the vicinity), and that on one of these occasions, when a severe shock was felt at Mendoza, the time at which the Cordoba clock stopped agreed with that observed at the points of chief disturbance, showing that the interval between the arrival of the tremors is less than the uncertainty of the watches. On a former occasion, when the Mendoza clock was stopped by a shock, a similar conclusion was arrived at.

COLOURS OF VARIABLE STARS[*].—In a paper read before the Boston Amateur Scientific Society, Mr. S. C. Chandler has taken up the important question of the relation between the colours of variable stars and the length of their period. Taking as a basis Schönfeld's second catalogue of variable stars, he finds, after deducting 26 of which the colours are not assigned, or the variations are irregular, 112 periodical variables, which he divides into five classes, according to the length of their periods, separating those which partake of any tinge of red from those noted as white, yellow, or of no perceptible colour. The results are shown in the following table:—

	White or Yellow.		Red or reddish.	
Periods.	No.	Per cent.	No.	Per cent.
Under 100	14	52	13	48
100–200	2	22	7	78
200–300	6	26	17	74
300–400	3	8	34	92
Over 400	0	0	16	100

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[*] Science Observer, 1878, July.

"The general result is, that as the length of the period increases, the proportion of colourless stars decreases, while that of the red stars increases."

Mr. Chandler has also arranged his material in another form, classifying the stars according to their colour and computing the average period for each class, with the following results:—

Class.	Colour.	Number.	Average Period.
1......	White	9	126 days.
2......	Yellow	16	124
3......	Yellow-red	21	212
4......	Red	47	288
5......	Intense red	19	77

"The progressive increase in the value of the average period from the 'white' stars at one end of the scale to the 'intense red' at the other end is very remarkable;" and Mr. Chandler does not think it can be merely apparent, depending on the mode or order of discovery.

LEYTON ASTRONOMICAL OBSERVATIONS, VOL. IV.—Mr. Barclay's Observatory at Leyton, Essex, has for upwards of 15 years been devoted to the observation of double-stars; and the present volume contains the measures made from 1873 to the end of 1877, together with the results of observations from the commencement, which have been included, as the earlier volumes are now out of print. In the Introduction a short description is given of the instrument used, a 10-inch Cooke Equatoreal, and some discussions on the value of a revolution of the micrometer-screw, and on the probable errors of observations of position-angle and distance reprinted from former volumes, are added. Classifying the stars according to distance, 0″–5″, 5″–10″ &c., and reducing the probable error in position-angle to its equivalent in arc, Mr. Barclay finds W. Struve's law confirmed, "that the probable error in angle and distance increases with the distance of the double star," though the increase is much more rapid for the angle. For distances of 5″ to 10″ Mr. Barclay finds the probable errors in the two coordinates equal, viz. 0″·1, whilst Mr. Struve measured position-angles more accordantly than distances up to the extreme distance of 32″. These determinations were made with a parallel-wire micrometer. With a double-image micrometer, the probable error was less, especially in measures of distance, a circumstance which Mr. Barclay attributes to their being independent of irregularities of the clock-motion in the case of the double-image micrometer. In the observations the measures with the latter do not appear to be distinguished in any way.

The observations of double stars made in the five years 1873–1877 are somewhat meagre in quantity, 67 stars only having been observed on an average twice each; but the Notes which follow are very complete, giving a most valuable summary of observations made from the time of Sir W. Herschel, and thus presenting at a glance the evidence for the relative motion or fixity of the components. Phenomena of Jupiter's satellites, and Occultations of stars (e printed, for the most part, from the earlier volumes), together with some observations of comets, complete the tale of observations.

PROPOSED OBSERVATORY IN JAMAICA.—Mr. Maxwell Hall, who some time since erected a private observatory near Montego Bay, has now drawn up a well-considered scheme for the establishment of a central observatory on the hills near Kingston, Jamaica, with branch meteorological stations on the other island, for the purpose of distributing time-signals and giving storm-warnings throughout the West Indies. Mr. Maxwell Hall estimates the first outlay required as £3000, and the annual cost as £2000, and proposes that the sum should be raised by contributions (ranging from £50 to £200) from the various West-India Islands, which would all share in the benefits of the scheme. Further, as this system of tracing storms cannot fail to be of importance to the shipping of the United States, for the reason that the storms generally follow the course of the Gulf-stream, sweep the eastern coasts of the United States, and then proceed towards the British Isles, it is hoped that the United States would also co-operate in the maintenance of the Central Observatory.

DISCOVERY OF TWO MINOR PLANETS.—Prof. Peters announces the discovery of two small planets, one (No. 190) on Sept. 29, of the eleventh magnitude, and the other (No. 191) on Sept. 30, of the tenth.

THE planet discovered by Prof. Watson on Sept. 22 turns out to be the old planet Aurora (94). This was established on computing the elements of the newly-discovered planet.

IN our notice of Mr. Thomas Grubb we should have stated that for the last nine years he had not taken any active part in the business, which has been carried on by his son, Mr. Howard Grubb; and it is by him that the large reflectors and refractors alluded to were constructed.

EDITOR.

Double Stars for November.

Σ 208=10 Arietis. R.A. $1^{h}\ 56^{m}\cdot 8$, Dec. +25° 22′.

Mag. 6·2, 8·4. Colours, A yellow, B ash. Binary.

	°	″	
1874·93	39·0	1·4	Gledhill.
1878·08	43·03	1·16	Doberck.

Σ 228=259 Andromedæ. R.A. $2^{h}\ 6^{m}\cdot 4$, Dec. +46° 55′.

Mag. 6·7, 7·6. Colours, white. Binary, in rapid motion.

1875·20	311°·4	0″·52	Dunér.

Σ 227=ι Trianguli. R.A. $2^{h}\ 5^{m}\cdot4$, Dec. +29° 45′.

Mag. 5, 6·4. Colours, A yellow, B blue. Probably binary.

	°	″	
1874·10	75·5	3·7	Gledhill.
1877·91	75·91	3·94	Doberck.

o Ceti=Mira Ceti. R.A. $2^{h}\ 13^{m}\cdot3$. Dec. −3° 31′.

Mag. A var., B 9. Colours, yellow, pale lilac. Rectilinear motion. This is the famous variable star; period about 331 days; range of magnitude 2·5 to 9·5.

	°	″	
1824·63	……	115	Struve.
1877·12	82·2	118·2	Flammarion.

Σ 257. R.A. $2^{h}\ 16^{m}\cdot6$, Dec. +61° 0′.

Mag. 7·2, 7·7. Colours, yellowish white. Binary. Needs careful measurement.

1869·13	186°·3	0″·28	Dunér.

Σ 262=ι Cass. R.A. $2^{h}\ 19^{m}\cdot2$, Dec. +66° 52′.

Mag. 4·2, 7·1, 8·1. Colours, A yellow, B blue, C blue. A ternary system.

		°	″	
A B	1874·93	265·7	1·9	Gledhill.
	1876·22	264·02	…	Doberck.
A C	1873·96	108·2	7·9	Gledhill.

Σ 300. R.A. $2^{h}\ 37^{m}\cdot5$, Dec. +38° 57′.

Mag. 7·9, 8·1. Colours, very white. Binary.

1865·89	304°·3	2″·71	Talmage.

Σ 333=ε Arietis. R.A. $2^{h}\ 52^{m}\cdot3$, Dec. +20° 51′.

Mag. 5·7, 6. Colours, white. Probably binary.

1877·07	197°·7	1″·17	Spörer.

Σ 412=7 Tauri. R.A $3^{h}\ 27^{m}\cdot3$, Dec. +24° 4′.

Mag. 6·6, 6·7, 10·5. Colours, yellowish. A B binary.

		°	″	
A B	1873·94	232·0	0·4	Gledhill.
A C	1873·80	60·6	22·8	Wilson and Seabroke.

Σ 460. R.A. $3^{h}\ 49^{m}\cdot9$, Dec. +80° 22′.

Mag. 5·2, 6·1. Colours, A yellow, B bluish.

	°	″	
1875·90	27·5	0·7	Gledhill.
1876·12	27·5	...	Doberck.

Σ 518. R.A. $4^{h}\ 9^{m}\cdot8$, Dec. −7° 49′.

Mag. A 4, B 9·1, C 10·5, D 12, E 11·4. Colours, A very yellow. A B C a ternary system.

		°	″	
A B	1877·12	104·7	81·5	Flammarion.
B C	1877·12	120·0	2·±	„
A D	1877·12	148·0	37·2	„
A E	1877·12	339·2	109·9	„

α Tauri. R.A. $4^{h}\ 29^{m}\cdot0$, Dec. +16° 16′.

Mag. 1, 11·2. Colours, A golden.

1877·06	35°·5	114″·5	Flammarion.

Σ 566. R.A. $4^{h}\ 30^{m}\cdot5$, Dec. +53° 15′.

Mag. 5·1, 7·4. Colours, A yellow, B bluish. Binary. Common proper motion +$0^{s}\cdot002$ in R.A. and +0″·11 in N.P.D.

1863·24	299°·5	1″·68	Dembowski.

Σ 589. R.A. $4^{h}\ 38^{m}\cdot5$, Dec. +5° 4′.

Mag. 8, 8. Colours, yellowish white. Measures wanted. Probably a binary.

1873·98	303°·5	4″·4	Gledhill.

Σ 634. R.A. $5^{h}\ 2^{m}\cdot8$, Dec. +79° 6′.

Mag. 4·5, 7·9. Colours, A yellowish, B white. Probably binary.

1875·37	1°·4	20″·29	Flammarion.

η Orionis. R.A. $5^{h}\ 18^{m}\cdot4$, Dec. −2° 30′.

Mag. 4, 5. Colours, A white, B purplish white. Probably binary.

	°	″	
1874·10	86·5	1·16	Gledhill.
1878·08	87·3	1·12	Doberck.

Σ 728 = 32 Orionis. R.A. $5^h\ 24^m{\cdot}4$, Dec. $+5°\ 51'$.
Mag. 5·2, 6·7. Colours, yellowish. Binary.

1877·19	188°·9	0″·44	Spörer.

Σ 774 = ζ Orionis. R.A. $5^h\ 34^m{\cdot}7$, Dec. $-2°\ 0'$.
Mag. 2, 5·7. Colours, A yellow, B reddish olive.

		°	″	
A B	1877·19	151·5	2·38	Spörer.
	1878·08	154·4	2·37	Doberck.
A C	1877·17	9·3	60·3	Flammarion.
	1878·07	9·2	....	Doberck.

JOSEPH GLEDHILL.

Meteor Notes for November.

THE month of the *Leonids* and *Andromedes* is always attractive to the meteor observer. On the 13th–16th the morning sky should be observed for stray members of the former shower (though the Moon is only a few days past the full, and will probably render the display of this year a scarcely visible one), and the nights of the 25th–29th (especially the 27th) should be vigilantly watched for *Andromedes*. The radiant-points have already been fully and very exactly determined by Prof. Herschel at 149° +23° and 23° +43° respectively. It is in the latter stream that the chief interest is now centered; for Biela's comet, which appears to follow precisely the same orbit, returns to perihelion next year, and the ensuing approach of the earth to the node occurs when the condensed part of the meteor-cloud is less distant than at any return since 1872. Observations have shown that this system is not continuous throughout its orbit like the Perseids, for it fails to give an annual display of any brightness. A few of these *Andromedes* were seen, however, on Nov. 25–27, 1877, so that the particles are somewhat scattered along the orbit, and this year we may expect to see many of these slowly moving meteors—the precursors of the main stream. In this connexion it may be worth pointing out that there was an interval of 40 years (6 revolutions of Biela's comet) between the fine star-showers of Dec. 6, 1798 and Dec. 7, 1838, and that a similar interval brings us to the present year, when, however, the conditions appear to be slightly less favourable. A contemporary shower (of quick white meteors), with a radiant at 54° +48°, was detected by the writer on Nov. 27 last year, but the same stream had already been observed by Schram at Genf (Austria) during the great display of 1872. From 108 tracks recorded by him, 16 per cent. conformed to a centre at 52°·2 +49°·4; and Heis also appears to have seen it, for he gives a long-continued shower at 55° +45°, Oct.

17–Nov. 30 (St_{22}, 63 meteors). At Pola a radiant close to Capella, at 76°·5 +46°, was also noted on Nov. 27, 1872. Heis's new catalogue of radiant-points includes an apparently rich one at 24° +42°, Oct. 17–Nov. 15 (P_1, 140 meteors), close to γ Andromedæ, which almost exactly coincides with the position of the *Andromedes*; but the two showers can hardly have any connexion. It will be important to recover Heis's radiant early in November, and compare the meteors with the true *Andromedes* visible later in the month. During the first half of November there is also a shower at 98° +44°, and many *Taurids* are visible at this epoch and about Nov. 20–23. On Nov. 8, 1877, a strong radiant of exceedingly swift meteors was seen at 101° +73°, and on the 12th a position at 126° +40° was accurately shown by several meteors, one of which appeared stationary at that point. The former shower can hardly be the same as one noted by Zezioli at 111° +65°, on Nov. 30, 1867, who records:—"Dal punto 111° +65° partirouo moltissime piccole stelle in varie direzione, per la loro frequenza e brevità delle trajettorie impossibili a determinare" ('Effemeridi del Reale Osservatorio di Milano,' 1869).

Recent Observations.—At Bristol, cloudy weather has rendered work of this kind almost wholly impracticable. A clear sky on the morning of Sept. 26, however, allowed watching for nearly three hours, during which a radiant of very bright swift meteors was visible close to β Aurigæ at 87° +42°. A few paths were seen from another point at 127° +58° in Ursa, and just before daylight a feeble radiant came into play at 119° +15°. At 15^h 5^m a very swift *Aurigid* = ♃ was traced falling from 66° +8° to 61½° −3°. It left a streak of 7° upon the latter part of its course.

Mr. H. Corder at Chelmsford observed 98 meteors in September; but the radiants deducible from them are diffuse and uncertain. One good shower of slowish meteors was fixed on Sept. 26 at η Piscium 15° +16°, and there was an adjoining radiant of quick meteors from 3° +7°. Other showers were suspected at η Cygni, κ Cephei, δ Ursæ Minoris, η Herculis, α–η Cephei, α Pegasi, and in Lyra (288° +37°) and Camelopardus (130° +70°). On Oct. 4 a position of swift meteors was determined at 29° +26°.

Mr. V. Cornish, of Debenham, Suffolk, gives an account of two bright meteors, recorded by an observer there. Oct. 4, 11^h, = ♃: began at 353¾° +2°; ended at 4½° +12½°; not very rapid. Oct. 7, 9^h 15^m: a very fine meteor, throwing a shadow in spite of the moonlight, traversed a path from 338° −3° to 13° −6½°. When it started it did not much exceed Saturn; but when it burst, its brightness and size were nearly equal that of the Moon (then 11 days old). At extinction it emitted several sparks, falling downwards about 1°. [Mr. Cornish writes that the large meteor of Aug. 31, 11^h, described in last month's "Notes" as equal to 1-3rd ☽, is probably overrated. The altitude was 26° ±.]

W. F. DENNING.

Nov.	G. M. T.	PHENOMENA.
	h m	
15. F.	4 37·6	♃ tr. D. 20° 28′ S.
	8 11·2	♄ tr. D. 3 44 S.
	8 20·9	♈ transits.
	8 51	♃ sets.
	10 45·4	♅ tr. D. 12 27 N.
	13 55	♄ sets.
	15 13	2854 Oc. R. 358°
	19 22	☉ rises.
	23 26·6	♀ tr. D. 16° 48′ S.
16. S.	23 44·9	☉ tr. D. 18 48 S.
	0 37·4	☿ tr. D. 23 14 S.
	4 8	☉ sets.
	6 10	Twilight ends.
	5 49	♃ 2 Sat. Oc. D.
	10 26	π^2 Canc. Oc. R. 4°
17. S.	20 15	43 Leo. Oc.† 215°
☾	5 58·0	
18. M.	6 17	♃ 2 Sat. Sh. E.
	15 31	p^3 Leo. Oc. R. 335°
19. Tu.	5 3	♃ 1 Sat. Tr. I.
	6 15	♃ 1 Sat. Sh. I.
	7 23	♃ 1 Sat. Tr. E.
20. W.	5 51·9	♃ 1 Sat. Ec. R.
	23 32·2	♀ tr. D. 18° 35′ S.
21. Th.	0 49·8	☿ tr. D. 24 40 S.
	5 27·9	♃ 3 Sat. Ec. D.
22. F.	3	☾ Perigee.
23. S.		
●	21 10·5	
24. S.	5 39·1	♃ 4 Sat. Ec. D.
25. M.	5 56	♃ 2 Sat. Sh. I.
	6 33	♃ 2 Sat. Tr. E.
	4 5·0	♃ tr. D. 20° 6′ S.
	7 31·3	♄ tr. D. 3 46 S.
	7 41·6	♈ transits.
	23 38·3	♀ tr. D. 20 9 S.
26. Tu.	23 47·5	☉ tr. D. 20 59 S.
	1 2·0	☿ tr. D. 25 33 S.
	7 3	♃ 1 Sat. Tr. I.
28. Th.	4 56	♃ 3 Sat. Oc. D.
	5 0	♃ 1 Sat. Sh. E.
	8	♃ ☌ ☾ ♃ 0° 31′ S.
29. F.	20	♄ stationary.
30. S.	8 8	7697 Oc.† 146°
	23 45·0	♀ tr. D. 21° 28′ S.
Dec.		
1. S.	1 13·1	☿ tr. D. 25 51 S.
☽	4 37·7	
2. M.	6 0	♃ 4 Sat. Tr. I.
	6 21	♃ 2 Sat. Tr. I.
	9 40	15 Pisc. Oc.† 326°
4. W.	0	☾ Apogee.
	5 48·2	♃ 2 Sat. Ec. R.
	6 22	♃ 1 Sat. Oc. D.
5. Th.	5 53	♃ 1 Sat. Tr. E.
	6	♀ Sup. ☌ ☉
	3 33·1	♃ tr. D. 19° 40′ S.
	6 52·0	♄ tr. D. 3 43 S.
	7 2·2	♈ transits.
	7 51	♃ sets.
	9 24·8	♅ tr. D. 12 18 N.
	12 37	♄ sets.
	19 52	☉ rises.
6. F.	23 51·3	☉ tr. D. 22° 32′ S.
	23 52·0	♀ tr. D. 22 32 S.
	1 21·1	☿ tr. D. 25 34 S.
	3 50	☉ sets.
	5 55	Twilight ends.
	6 9	26 Ariet. Oc.† 333°
7. S.	18 23	17 Tauri Oc. D. 52°
	18 44	16 Tauri Oc.† 355°
8. S.	4	☿ elong. 20° 45′ E.
	9 38	χ' Tauri Oc. D. 62°
9. M.		
○	7 49·8	
11. W.	23 59·4	♀ tr. D. 23° 19′ S.
	1 22·8	☿ tr. D. 24 43 S.
	5 18	ω Gem. Oc.† 4°
	16 25	58 Gem. Oc. R. 261°
12. Th.	5 34	♃ 1 Sat. Tr. I.
13. F.	6 5·8	♃ 1 Sat. Ec. R.
	15 28	π' Canc. Oc.† 30°

† A near approach.

EDITOR.

Distances from Saturn's polar diam., in equatoreal semidiams.
— preceding or west, + following or east, N. north, S. south.

8h G.M.T.	Encel.	Tethys.	Dione.	Rhea.	Titan.
Nov. 15....	−0·4 N	−3·9 S	+3·3 N	+7·8 N	−13·1 S
16....	+4·0 S	+3·2 N	−6·3 S	−2·9 N	− 6·0 S
17....	−0·6 S	−2·5 S	+5·0 S	−8·8 S	+ 2·1 S
18....	−3·9 N	+1·6 N	−0·4 N	−0·3 S	+ 9·8 S
19....	+1·6 N	−0·7 S	−4·5 S	+8·7 S	+16·2 S
20....	+3·5 S	−0·2 N	+6·4 S	+3·4 N	+20·2 S
21....	−2·5 S	+1·2 S	−3·9 N	−7·5 N	+21·3 N
22....	−2·8 N	−2·0 N	−1·2 S	−6·1 S	+19·4 N
23....	+3·2 N	+2·8 S	+5·5 N	+5·3 S	+14·8 N
24....	+2·0 S	−3·6 N	−6·1 N	+8·0 N	+ 8·0 N
25....	−3·7 S	+4·1 S	+2·6 S	−2·5 N	+ 0·1 N
26....	−1·1 N	−4·6 N	+2·7 N	−8·9 S	− 7·9 N
27....	+4·0 N	+4·9 S	−6·1 S	−0·7 S	−14·6 N
28....	+0·1 S	−5·0 N	+5·5 S	+8·6 S	−18·9 N
29....	−4·0 N	+4·9 N	−1·1 N	+3·8 N	−20·1 S
30....	+0·9 N	−4·7 S	−4·0 S	−7·3 N	−17·9 S
Dec. 1....	+3·8 S	+4·3 N	+6·4 N	−6·4 S	−12·8 S
2....	−1·9 S	−3·8 S	−4·5 N	+5·0 S	− 5·5 S
3....	−3·3 N	+3·1 N	−0·4 S	+8·2 N	+ 2·5 S
4....	+2·7 N	−2·3 S	+5·1 N	−2·0 N	+10·3 S
5....	+2·6 S	+1·5 N	−6·3 N	−8·9 S	+16·5 S
6....	−3·4 S	−0·6 S	+3·3 S	−1·2 S	+20·3 S
7....	−1·7 N	−0·4 N	+2·0 N	+8·5 S	+21·3 N
8....	+3·8 N	+1·3 S	−5·9 S	+4·2 N	+19·2 N
9....	+0·8 S	−2·2 N	+5·8 S	−7·0 N	+14·5 N
10....	−4·0 S	+3·0 S	−1·8 N	−6·7 S	+ 7·6 N
11....	+0·3 N	−3·6 N	−3·4 S	+4·6 S	− 0·3 N
12....	+4·0 S	+4·2 S	+6·3 N	+8·4 N	− 8·2 N
13....	−1·3 S	−4·6 N	−5·0 N	−1·6 N	−14·8 N
14....	−3·6 N	+4·9 S	+0·3 S	−8·9 N	−19·0 N
15....	+2·2 N	−5·0 N	+4·6 N	−1·6 S	−20·0 S

Semiaxes of orbits expressed in semidiameters of Saturn's equator.

Dec. 1.	Mi.	En.	Te.	Di.	Rh.	Ti.	Iap.
Major	3·14	4·03	4·99	6·40	8·93	20·71	60·34
Minor	0·09	0·12	0·15	0·19	0·27	0·56	8·29
Hourly Motion	15°·9	10°·9	7°·9	5°·5	3°·3	0°·9	0°·2

Iapetus is near its superior conjunction, south of the planet, on Nov. 19, and at its greatest eastern elongation on Dec. 9.

A. M.

Satellite of Neptune.

Major and minor semi-axes of apparent orbit on Dec. 1, 16″·9 and 5″·7. The satellite is near its greatest *n.f.* elongation in pos.-angle 40°·5, and near its greatest *s.p.* elongation in pos.-angle 220°·5, at the following times:—

Nov. 15	11h	*n.f.*	Nov. 24	7h	*s.p.*	Dec. 3	2h	*n.f.*	Dec. 11	22h	*s.p.*
18	9	*s.p.*	27	5	*n.f.*	6	1	*s.p.*	14	20	*n.f.*
21	8	*n.f.*	30	4	*s.p.*	8	23	*n.f.*	17	19	*s.p.*

A. M.

Position of the Sun's North Pole.

1878.	Pos.-angle. °	'	Dist. from limb. °	'
Nov. 16........	21	7	+2	28
21........	19	35	1	52
26........	17	52	1	14
Dec. 1........	15	59	+0	36
6........	13	57	−0	2
11........	11	48	−0	40

The Moon.

1878.	Rises. h m	Geoc. Libr. at midnight. long. °	lat. °	1878.	Sets. h m	Geoc. Libr. at midnight. long. °	lat. °
Nov. 16...	10 1	−6·03	+0·93	Dec. 1...	11 48	+4·24	−4·01
17...	11 22	5·61	2·44	2...	12 57	2·99	5·02
18...	12 45	4·85	3·85	3...	14 5	1·67	5·81
19...	14 8	3·72	5·05	4...	15 14	+0·37	6·34
20...	15 37	2·27	5·96	5...	16 25	−0·85	6·61
21...	17 6	−0·57	6·47	6...	17 37	1·95	6·59
22...		+1·26	6·55	7...		2·89	6·26
23...	New.	3·02	6·19	8...		3·66	5·64
24...		4·59	5·42	9...	Full.	4·25	4·74
25...	Sets.	5·81	4·32	10...		4·65	3·59
26...	5 30	6·59	2·98	11...	Rises.	4·89	2·22
27...	6 47	6·90	1·51	12...	6 29	4·94	−0·73
28...	8 7	6·76	+0·01	13...	7 50	4·81	+0·83
29...	9 24	6·22	−1·45	14...	9 12	4·49	2·37
30...	10 38	+5·35	−2·81	15...	10 33	−3·95	+3·79

The libration-curve is interesting on account of its approach to circularity. On Dec. 8 the remarkable depressions on the Moon's preceding limb will again be visible, and the changes in their aspect ought to be watched and measured on that and the preceding and following night.

A. M.

The Planets.

Mercury is an evening star, at greatest elongation east (20° 45′) on Dec. 8; when he sets $1^h\ 15^m$ after the Sun.

Venus is a morning star till Dec. 5, when she is in superior conjunction with the Sun. Her diameter is 9″·8 throughout November and December.

Mars is a morning star, moving *s. f.* through Libra, and rising from 2^h to $2\frac{1}{2}^h$ before the Sun.

Jupiter is an evening star, setting about 8^h.

Saturn is stationary about midway between α Pegasi and β Ceti, setting after midnight.

	Outer Ring. Maj. Axis.	Outer Ring. Min. Axis.	Inner Ring. Maj. Axis.	Inner Ring. Min. Axis.	Ball. Diam.
Nov. 21	41″·54	1″·26	27″·62	0″·84	16″·7
Dec. 11	40″·14	1″·29	26″·68	0″·86	16″·1

The south side of the rings is visible, the elevation of the Earth above their plane being 1° 44′ S. and 1° 50′ S. on Nov. 21 and Dec. 11 respectively, and of the Sun 4° 19′ S. and 4° 37′ S.

Neptune is an evening star, nearly midway between α Arietis and α Ceti.

EDITOR.

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THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 20. DECEMBER 1. 1878.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 8th November, 1878.

Lord Lindsay, F.R.S. &c., *President*, in the Chair.

Secretaries: Mr. J. W. L. Glaisher, F.R.S., and
Mr. A. Cowper Ranyard.

The Minutes of the last Meeting were read and confirmed.

Mr. Glaisher reported that 133 presents had been received by the Society since their last Meeting in June, and amongst them a copy of the 'Sidereal Messenger,' presented to the Society by Mr. S. W. Burnham, the most complete copy in England, and also a copy of Dr. Schmidt's Map of the Moon, presented by the Prussian Government. Thanks were voted to the donors of those presents, especially of the two latter.

Mr. David Gill read a paper on "The results of Meridian Observations of the Mars Comparison Stars." He said, "This paper is only suitable for printing, and I will give but a brief *précis* of it to the Meeting.

"Before leaving England for Ascension I applied to the principal Observatories for cooperation, and I desire now to record the hearty response which has been made to my appeal and to return my warmest thanks to the Directors of the various Observatories for their valuable assistance.

"From three observatories the results have not yet been received, but from twelve observatories the results are in my possession. These results I have discussed, with the result of finding a very peculiar error, due, apparently, to difference of the habit of observers in noting the transits of stars of different magnitudes. Thus the Right Ascensions at Washington required a mean systematic correction of $-0^{\mathrm{s}}{\cdot}120$ and those at Oxford of $+0^{\mathrm{s}}{\cdot}076$ to reduce them to the mean system derived from all the observatories. Only a small part of these discordances ($\pm 0^{\mathrm{s}}{\cdot}03$) could be traced to systematic difference in the places of the clock stars employed, and there were many reasons for supposing them due, in great part, to the cause above mentioned.

"I accordingly addressed a circular to the various observatories requesting certain observations, with a view to settle the question and to determine, if possible, the absolute R.A. of the comparison stars.

"The matter has been energetically taken up at Leiden, Pulkowa, Cambridge, U.S., and Greenwich, and I hope also at other observatories.

"At Leiden it has been shown, by observing alternately with a diminished aperture and a full aperture, that the R.A., is affected systematically by the magnitude, the results for the three observers being:—

$$\left.\begin{array}{l} -0^{s}{\cdot}073 \\ -0\ {\cdot}025 \\ -0\ {\cdot}051 \end{array}\right\}\ \begin{array}{l}\text{For a difference corresponding with} \\ \quad\text{a difference of } 2\tfrac{1}{2} \text{ magnitudes in} \\ \quad\text{brightness.}\end{array}$$

"I have, however, discussed the observations as they stand on the supposition that the results of each observatory are affected by three errors:—

"1. A systematic error common to all the stars.

"2. By a probable error depending on the square root of the number of observations of any given star.

"3. By a probable error peculiar to each star not diminished by any number of observations.

"These errors having been determined, the first eliminated, and a system of weights calculated from the second and third, the most probable place of each star was deduced. The discussion led to some interesting conclusions which can only be followed by a perusal of the paper in print."

Mr. Gill then said, "I have now to bring before the Society 'A report on the Observations of Mars made at Ascension,' and I have done so in the present form at the suggestion of the Astronomer Royal.

"A long time must elapse before I can give the definitive result of the whole of the observations, because so many investigations have sprung out of the original one, investigations that require more observations, and hence more time, for their satisfactory discussion. This being so, and being continually asked by Fellows when the results will be published, I felt it was due to the Society to show that no time was being lost, and to give some idea of the prospects of a satisfactory result.

"I have therefore discussed the observations of Mars from Sept. 4 to 11 inclusive, and this is the subject of the present paper."

Mr. Gill then showed how the chronometer error, latitude and longitude had been determined, how the tabular quantities had been computed, how the observations had been made, and the various instrumental corrections had been determined and applied. He proceeded to show how very desirable it was that the star-places should be determined with extreme precision, so that they might be treated as absolutely known in the final discussion. He showed how, by employing only the method of corresponding evening and morning observations, many valuable observations were lost, and the door was opened for an arbitrary selection of observations and arbitrary treatment of results, which was most undesirable. He also showed the great value of well-determined star-places in correcting the tabular motion of the planet.

The energetic cooperation of the various observatories had enabled him to assign, even now, places of the comparison stars whose probable error did not much exceed ± 0″·1 of arc, and he hoped when all the observations had been received, and the whole had been fully discussed and combined with the heliometric triangulation, the probable errors of the star-places might be obtained with an error not much exceeding ± 0″·05. Employing the star-places arrived at in the previous paper, and rejecting no complete observation whatever between Sept. 4 and 11, the probable error of the measurement of the distance between the planet and a star is ± 0″·30, and this includes both the error of the star's place and the error of the heliometric measurement.

It would thus seem that a single observation of Mars by the heliometer is as accurate as an average observation of contact at a Transit of Venus, whilst the parallax factors are quite as favourable. Thus on a fine night, at a suitable station, a single observer could do as much towards the determination of the Solar Parallax as nine or ten costly Transit of Venus Expeditions, and his observations have, moreover, the enormous advantage that they are capable of only one interpretation.

The probable error of the resulting parallax was ± 0″·017, and this had been derived from 104 observations; as there were in all about 360 observations, a final result of very great accuracy might reasonably be looked for.

Mr. Gill then said, "Beyond the reasons, already stated, that induced me to lay this paper before the Society in its present form, there is the hope that now or at some future time I may have the benefit of criticism on the methods I propose to employ in deducing the definitive result. There is only one form of criticism which is useless, and unfortunately it is a common one, viz. that the result is right or wrong according as it agrees with the critic's preconceived notions as to what it should be. (Laughter.) It is for this reason I have not published the assumed value of the parallax, but have exhibited only the correction to the assumed parallax.

"All honest and useful criticism can equally well be made without this information; and therefore I have thought it better to defer the publication of any result until the final definitive result has been arrived at."

Mr. Gill then read "A note on some remarks of Mr. Maxwell Hall." These remarks were published in the last number of the 'Monthly Notices,' and Mr. Hall claims superiority of the method of observing transits of Mars over the heliometric method.

Mr. Hall used as an argument in favour of the accuracy of his method that the changes of his clock-rate were shown in intervals of 10 minutes or so. He makes the rather extraordinary statement that though his clock may be depended upon to ½ a second a day, yet its rate varies in ten-minute intervals as much as 2 or 3 tenths of a second.

Mr. Gill contended that it was impossible to distinguish between clock-rate, and error of observation in this case, and that being so, Mr. Gill, from what he knew of clocks, thought it much more probable that Mr. Hall's observations were in error, rather than the clock-rate.

If it were not so, Mr. Hall must have been singularly unfortunate in his clock. Any moderately good German clock beating seconds, which could be bought for 5*s*., might certainly be relied on for a greater accuracy, and any old 8-day English kitchen-clock, with a seconds pendulum, might be relied upon for six times the accuracy in question.

The Astronomer Royal. With regard to the very important communication which has just been made to us, I think Mr. Gill has indicated that the possible difference of personal equation in the observations of stars of different magnitudes is likely to be important in questions of this kind. I was not aware of it until Mr. Gill was good enough to communicate that point to me a few days ago. I have not had an opportunity of verifying it; but, if possible, I will take an early opportunity of doing so with regard to some of those stars which are most interesting to Mr. Gill. I would further observe that when I made a communication to the Society about twenty years ago (I forget the precise year) I spoke of various methods of finding the parallax of the Sun, and I advocated the determining of the difference of right ascension between Mars and stars east of the meridian and west of the meridian as, upon the whole, what I considered the best. At that time there had been no good instance of the use of the heliometer in England, and certainly there had been no instance of its employment for any length of time. There were, at that time, a far smaller number of small planets exterior to Mars than there are at the present time, and there were none whose orbits fell so nicely near the orbit of Mars as some of those now known. Under the considerations which were before my eyes at that time, I was well justified, and, perhaps, I should still have been justified, in recommending the comparison of the position of Mars with the neighbouring stars as the best that could be adopted. But a new consideration has strongly suggested itself to me, that when you observe Mars with a star, you observe Mars under the circumstances of atmospheric refraction and dispersion of light, and you observe the star under circumstances which, geometrically considered, are the same, but which, considered with reference to the dispersion of the light, may not be the same; and those differences are prejudicial because they always operate in the same way on observations made on the east side and on the west side of the meridian. As regards the ultimate determination of the parallax there is no getting rid of them. It is, I think, an important thing, which has been introduced, so far as I know, by Mr. Gill, that, instead of observations of Mars, observations of the smaller planets which come near to Mars have been introduced.

Mr. Gill. I think Dr. Galle was the first who suggested that.

The Astronomer Royal. I think that is important, because the aspect of a star and the aspect of a small planet are so much the same, that it is probable that the atmospheric dispersion would be the same for both, and that would affect distinctly the difference of right ascension between the morning observations and the afternoon observations, upon which the determination of the solar parallax entirely depends. I would also say, with respect to some of the later points, that I think Mr. Gill has just mentioned what appears to be an approximate idea of the alteration to be made in the hitherto received magnitude of the solar parallax, and I think he exercises a wise discretion in suspending any thing like an official declaration until his reduction has been completed.

Mr. Penrose read a paper on the solar eclipse in July last, not materially different from his account of his observations in the 'Observatory,' No. 19. He also exhibited a drawing of the Corona, in which he had incorporated, with the results of his own observations, the streamers seen by Prof. Cleveland Abbe, Dr. Edmunds, and Mr. Lockyer. In a diagram he further showed how these streamers might be formed by meteor-streams circulating round the Sun.

Mr. Schuster. I have to offer some remarks on the observations made during the last total solar eclipse, and also some theoretical considerations which I hope will assist in the discussion of other observations. The observing station which I chose was in South Colorado, at a small village called Las Animas. I observed with a 4½-inch achromatic telescope, for which I was indebted to Col. Campbell of Blythswood, and with an ordinary spectroscope having a prism of dense flint and 60° angle. Unfortunately I had an accident with the fluorescent eyepiece with which I intended to have studied the ultra violet, so that on the day of the eclipse I had to change the plan of observation and to confine myself to the visible part of the spectrum. As the eclipse drew on, I covered the left eye with a piece of black velvet, and all the observations which preceded the eclipse have therefore been made with the right eye. For delicate observations this is not unimportant. As an experiment I looked at a sheet of ordinary white paper about half an hour before the eclipse, both with the eye which had been uncovered during the same time and also with the eye which had been covered; and the paper which looked to the one eye a dull white was so dazzling to the other that I could not bear to look at it. Whilst observing the spectrum as the eclipse proceeded, I was greatly struck with the fact that the blue and the violet lines of the spectrum came out with much greater intensity than they had done before. At the time I thought that it was merely the effect of contrast, that the general illumination had decreased, and that therefore the eye was more sensitive to the blue. So much struck was I with the fact, however, that I took notes about it until ten minutes before totality, when I could see the violet beyond the line K. After totality I spoke to Prof. Eastman, who was observing close by me, and showed him my notes, and he told me that he had been struck by the same fact. If you read Prof. Young's account of the eclipse you will find that before totality he could see the ultra-violet lines with a fluorescent eyepiece up to O better than he could do before the eclipse. I cannot conceive that this is merely the effect of contrast. We know that the atmosphere is sometimes transparent to the blue rays and sometimes very opaque. It is conceivable that as the eclipse went on the atmospheric conditions were changed from the one to the other. We know that aqueous vapour is opaque to the ultra-violet, and that water is very transparent to the same rays. After quartz, water is the most transparent body for the violet rays, and it ranks in this respect higher than Iceland spar. During the eclipse the aqueous vapour in the atmosphere may have partly condensed owing to the reduction of temperature, and we might thus explain the greater intensity of the violet rays as the eclipse went on. From ten minutes before totality I watched with one tube of the opera-glass to see whether the corona was visible before totality; but totality was close at hand when I could make out the corona. I saw it shoot out on one side, and I at once took up the beat of a chronometer which was close by me, and counted up to the time that the Sun finally disappeared, and I counted 6 seconds. I am sure that the corona was not visible at the station where I observed until about 6 seconds before totality, but I am sure it was visible at that time. As soon as totality began I went to the spectroscope. I had previously asked Mr. Haskins, who pointed the telescope for me, to put the finder on a strong prominence, or, if he saw none, on to the brighest part of the corona. He put it on the brightest part of the corona; the spectroscope was pointed to the violet, and I was startled by the great intensity of the violet end of the spectrum. No lines, either dark or bright, were visible. I measured the extent to which the blue end was visible, and I could see as far as wave-length 4070, which is further than the hydrogen line *h*. I narrowed the slit; but apparently the width of the slit had no appreciable effect upon the limit of the spectrum. I then asked Mr. Haskins to move the telescope further away from the corona, and he moved it three tenths of the semidiameter away; but I could find no difference in the intensity of the spectrum. I then moved the telescope into the green, and expected to find the green line, but I could not find it. I looked again, and I think I saw two faint green lines in the green; but before I could put the pointer of my eyepiece on to them to make sure of them the Sun reappeared. Prof. Eastman, who observed close by me, afterwards told me he had seen a green line, but he also suspected another line in the green a little more refrangible than the line 1474. I believe I have seen the green line 1474, although I am not sure; but there can be no doubt that the line was there, though faint. I saw no dark lines, neither were they seen by Prof. Eastman or Prof. Young, who worked with a very narrow slit. That the dark lines existed I have no doubt, from the observations made by Dr. Henry Draper's party; but I should say, from my own observation, that they must have been weak. I give this as evidence that the continuous spectrum is not due to simply reflected sunlight, but the particles are also self-luminous, a thing which you might expect so near the Sun. The polariscope is likely to give us further information as to the reflexion of this light from the corona. There have been a good many observations about the polarization of the corona, but there has been no rigorous calculation hitherto about what that polarization ought to be. It ought to increase away from the Sun, but the exact way in which it ought to increase was not known. I have made a calculation about the light scattered from the small particles in the neighbourhood of a luminous sphere, and I find that the percentage of polarized light is

$$3\sin^2\phi\frac{\cos\omega+\cos^2\omega}{4+\cos\omega+\cos^2\omega}.$$

This gives the law according to which the polarization ought to increase. ϕ is the angle subtended at the scattering point by lines drawn to the centre of the sphere and to the observer's eye; ω is the angle subtended at the same point by a line drawn to the centre of the sphere and a tangent to the sphere. It appears that a particle close to the Sun does not polarize the light at all, and that the polarization steadily increases as the point is removed from the Sun. M. Prazmowski, in 1860, found that the polarization reached its maximum some way from the Sun, and decreased on either side. M. Janssen, in the year 1871, confirmed this fact. In the same year (1871) we had an observer in India, Mr. Winter, who found that the polarization was mnch smaller close to the limb than it was some distance away. During the present eclipse Prof. Wright measured the amount of polarization in the corona; but he only began 5′ away from the Sun, and he found a rapid decrease of polarization after that. The increase of polarization is easily explained, but the decrease not so easily. The particles being hotter near the Sun than further away from it, they would increase the light nearer the Sun, and therefore render the relative amount of polarization still smaller at the Sun. The only explanation which would account for the decrease is, that beyond a certain point the particles are too large to polarize the light, but reflect it according to the ordinary laws of reflection.

There is one more consideration to which I wish to draw the attention of the Society. It has often been remarked that the corona is symmetrical round the axis; sometimes it has been said it is symmetrical round the ecliptic, and the Sun's equator and the ecliptic are inclined at an apparent angle varying from 7° 15′ down to 0. Calculating the amount of inclination for the various eclipses of which we have exact drawings, I find that in the eclipse of 1871 the inclination was more than 7°, and in 1870 it was nearly 7°. Then we have got the eclipse in Siam, where it was over 4°. Mr. Stone at the Cape of Good Hope had about 5°; and this time in America I think it will be between 4° and 5°. If we could refer to the old eclipses, especially the one in 1871, we might get further information upon this subject. I had only time to look at it very slightly a few days ago, and I found that the axis of symmetry does not quite agree with the Sun's equator and still less with the ecliptic. On the hypothesis of meteor-streams there is no reason exactly why it should be in the ecliptic, even though there might be a reason why it should be inclined at a small angle to the ecliptic. I want to point out, however, not only this symmetry, but a departure from symmetry which is observed in various eclipses. Take the eclipse of 1874 at the Cape, and the one of 1875 in Siam. The equatoreal streamers are wider and less divided up towards the east than they are towards the west. From drawings made in 1868, a similar but opposite thing was observed then; and it is, I think, significant that the relative position of Sun and Earth in 1874 and 1875 was approximately the same, while in 1868 the longitude was about 120° different.

I took the axis of symmetry for the 1871 eclipse from a photograph of Col. Tennant's in the 'Memoirs' of the Society; I then took the north point as given by him, and also the axis of symmetry as shown upon the photograph. The equator was inclined at an angle of 7° 13′ to the ecliptic.

Mr. Marth. That is a roundabout way of doing it; you need not trouble yourself about the ecliptic.

Mr. Schuster. If I want to compare the Sun's equator with the ecliptic, I must do *both* and must calculate the position of both.

Mr. Ranyard. There is some little doubt as to what is the position of the axis of symmetry. I should be sorry to say definitely within 5° where the axis of symmetry lies; but there is certainly a difference in the inclination of the axis of symmetry to the Sun's axis in the different eclipses: the exactitude with which the position of the projection of the Sun's axis can be determined differs very greatly; it cannot be laid down at all upon the photographs, unless reference-lines are given. In 1871 such a reference-line was provided by Lord Lindsay's assistant Mr. Davis. After the eclipse he exposed a plate and allowed the Sun to move onward by the rotation of the Earth, and thus traced upon the plate a line parallel to the equator of the heavens. A series of pictures of the Sun were taken at intervals of about half a minute, and from these the position of the equator of the heavens was laid down upon the plate, and a corresponding line was laid down upon the plates upon which the negatives of the corona were taken. This operation could not be performed within, say, 1° of accuracy, though probably it would certainly be within 5° of the truth. I repeated the operation many times, and have no doubt that, in the case of Lord Lindsay's plates, the axis has been determined more accurately than with any others; but when we come to the eclipses of 1870 and 1874, I should be sorry to speak with such certainty. In order to determine the angles between the Sun's axis and the north point, two things have to be taken into account—one the position which the Earth has with regard to the node of the Sun's equator, and the other the angle which the ecliptic makes with the equator in the heavens. The pole of the ecliptic may be as much as 23° away from the north point, and the Sun's equator may make an angle of 7° with the ecliptic. With regard to polarization the question is one of considerable interest, which I have gone into in the Eclipse Report; I come to rather a different conclusion, viz., that the decrease in the intensity of polarization near the limb might be due to the particles in the lower part of the corona being incandescent. Light from an incandescent body is always unpolarized, and the light of the incandescent particles, mixing with the polarized light, might tend to make the polarization less in the lower area. Mr. Schuster mentioned four or five observations which tend to show that there is an area of maximum polarization, not at the Sun's limb, but at some little height above it, from 5′ to 10′. During the recent eclipse there was not such a definite area of maximum polarization as I expected; and what astonished me most, was the way in which the polarization could be detected right down into the limb. There was an area of maximum polarization, but it was not nearly so marked as I expected to find it.

Mr. Schuster. We do not know in what way the quantity of scattering matter varies with the distance from the Sun, and therefore the integration which Mr. Ranyard said he did cannot be done; but we should take into account every element of the Sun's surface. The polarization ought regularly to decrease outward. I do not see that you can possibly integrate the elements along a line of sight, because you do not know in what way they are distributed along that line.

Capt. Noble. I see it stated in the paper that the corona was 20 times less bright than in the year 1870. Can Mr. Penrose tell me whether it is the fact that the corona was so very much dimmer upon this occasion?

Mr. Penrose. I unfortunately did not see the corona in 1870, but I know that the light on this occasion was very much greater than it was in 1870. In 1870, although a considerable part of the sky was clear to me, and I saw all the effect of the shadow rushing over and its effect upon the clouds, yet the actual Sun and Moon were hidden from me; there was then very great darkness. On that occasion I had great difficulty in reading tolerably easy print; whereas on this occasion I had no difficulty whatever with the fine pencil-lines of my notes.

Mr. Ranyard read a paper by Prof. Piazzi Smyth on some measures of the lines of the B group in the spectrum of a high Sun. During the past summer Prof. Piazzi Smyth had visited Spain, in order to observe the absorption lines at the red end of the spectrum. He describes the B line, which is usually given as a broad black line, a breaking up into a bundle of fine lines.

A paper from Mr. Burnham of Chicago, which is to be printed in the 'Memoirs,' was also referred to. It contains a list of the double stars which Mr. Burnham has discovered and observed with the Dearborn 18½-inch equatoreal during the past year and a half.

Mr. Neison. In a recently published volume forming one of the publications of the Washington Observatory, Prof. Newcomb has reduced a number of ancient observations of the Moon, and from them derived a correction to the secular acceleration of the mean motion of the Moon. In this way he obtains for the secular acceleration indicated by observation a value of 8″·31. It will be remembered that from the investigation by Delaunay and Prof. Adams it was found that the theoretical value was only equal to 6″·18; whilst Hansen in his papers had given reasons for believing that the observations indicated a value of 12″·18. It has been generally believed that this difference between these two values was due to the friction of the tides upon the Earth, which caused the Earth to be retarded in its rate of rotation, so that the days were rendered a little bit longer. Prof. Newcomb now shows that the observed value is only 8″·31, or very much closer to the theoretical value, showing that if there was any tidal retardation it must be very much smaller than was assumed. In looking over Prof. Newcomb's paper it struck me that his observations had given very great weight to the most ancient eclipse of all, the lunar eclipse which occurred B.C. 720 in Babylonia. I find that that eclipse was discordant with all the other eclipses; for whereas the other eclipses indicated a very large correction to the value assumed by Hansen, this particular eclipse indicated a value pretty nearly equal to it. It struck me that it would be advisable to consider what would be the effect of leaving out this discordant group of eclipses, and I found that it at once reduced the value of the secular acceleration from 8″·31 to 7″·20. By this means the most probable value indicated by observation differs only by one second of arc from that obtained by theory; and this circumstance throws a great deal of doubt upon the supposed retardation in the rotation of the Earth.

Mr. Ranyard read a paper on the presence of particles of meteoric iron in the atmosphere.

In a paper by Mr. John Murray, published in the Proceedings of the Royal Society of Edinburgh, on the deep-sea mud brought home by the 'Challenger' expedition, he describes numerous small particles of iron which appeared to have a meteoric origin. When they were extracted by means of a magnet, and moistened under the microscope with an acid solution of sulphate of copper, he found that the copper was deposited in ramified markings and lines which reminded him of the structure seen in meteorites. When a meteorite is polished and etched, it shows a crystalline structure which is very characteristic. Mr. Murray found that the copper deposited itself upon the little spherules in lines which reminded him of these characteristic markings. He also found several small brown nodules with a mamillated outer surface like the Cape meteorite; he was so struck with the peculiar surface of those mamillated nodules, and the strange lines in which the copper was deposited, that he felt forced to believe that the iron had a cosmic origin. He came to the conclusion that the reason why these little masses of iron were found at great depths, and not found in such quantities in mud brought from shallower depths, was that in the very deep seas there is very little to fill up and to obliterate the cosmic dust, while that which falls into the estuaries of rivers or within some hundred miles of them is lost in the matter brought down and continually being deposited. Prof. Alexander Herschel has, I understand, seen these small iron particles, and is convinced of their cosmic origin. Besides this a Swedish mineralogist exposed some glycerine plates to the wind, and on examining the dust collected on the plates found small particles of iron. It has been suggested that possibly these particles might come from iron-works or from forges in some neighbouring town; and I thought it worth while, on returning from America, to attempt to collect some dust out at sea. Dr. Draper was good enough to give me a box to hold the plates and some glycerine which I found was perfectly free from iron; the plates were exposed when we were a thousand miles from land, some of them for 30 hours, and some for 18 hours, having first been cleaned and covered with pure glycerine. Since my return I have examined them microscopically, and on one of them I found a particle of iron, perhaps the $\frac{1}{120}$ or $\frac{1}{150}$ of an inch in its longest diameter. I am not quite satisfied with the experiment, because on putting the plates into the box it was not properly cleaned. The sides should have been covered with glycerine so as to prevent dust from the box falling on the plates. My object in bringing the matter before the Society is to try to induce somebody else to undertake a similar experiment with greater precautions. Not only should the chemicals be tested, and the plates properly cleaned, but the inside of the box should be covered with glycerine, and a day or two should be allowed before the clean plates are put in, so that if there is any dust it may be deposited upon the glycerine in the box. I should say that the wind was not directly ahead of us, but still it was not blowing from the direction of the funnel or from the ship.

Mr. Gill. I thought that the dust found in those cases of mud was very much more microscopic in character; you describe it as something which you could not imagine as being carried by the wind.

Mr. Ranyard. I found some other traces of iron, but it was mostly in connexion with minute hairs or minute sea creatures. All the animal matter that I found upon the glycerine seemed to contain iron and to give a red reaction.

Dr. Draper was good enough to give me, to present to the Society, an original negative of the spectrum of the Sun and the spectrum of oxygen. It will be remembered that Mr. Christie made a map of the part of the solar spectrum in the neighbourhood of the G line, and he found a number of lines that were not shown in a paper print which was published by Dr. Draper in the pamphlet announcing his discovery of oxygen in the Sun. Mr. Christie found that the bright lines which were shown in that paper print were, when examined with greater dispersion, broken up, and he thought it was improbable that the oxygen lines would themselves be broken up in the same manner so as to correspond with these solar lines. Mr. Christie mapped a small portion of the spectrum, which I have very carefully examined with high dispersion by means of a grating of 17000 lines to the inch. There can be no doubt that the bright solar lines are very much broken up indeed. But the chief reason why Dr. Draper gave me this photograph was in order that I might show to the Society a certain group of four unequal lines of oxygen; it will be seen that they match with four similar lines in the solar spectrum. A great deal more is shown in this photograph than is shown in the paper one, which was printed by some automatic method and was published with Dr. Draper's paper. It will be seen, on examining this, that a line that appeared single in the print is broken into four lines of very different intensities; and not only do these four lines match in position, but they seem to match also in intensity with the intensity of the bright lines of the oxygen spectrum. Dr. Draper's expression about it was, that he would be willing to rest the whole case on those four lines. The chances of such coincidences in position, taken together with the chances of the relative intensities matching as they do match, would, I think, be enough to prove the case, And I think that is borne out when you take into account what is the probability of the lines matching as these lines do. I believe I should not be overstating the case if I said that the chance against their matching in position as they do was 8 to 1, but let us say 6 to 1; then the chance against four such coincidences would be 6^4 to 1, or more than 1200 to 1, and then we have to take into account the matching of the relative intensities.

Mr. Christie. Mr. Ranyard has rather misunderstood my reasoning with regard to this question of the lines. The chief point on which I insisted was that, whereas in Prof. Draper's photograph the so-called bright lines were fuzzy and quite as broad as the spaces between them, in the photographs which we have taken at Greenwich, and also in Dr. Rutherfurd's fine photographs of the spectrum, the dark lines, as we have been in the habit of calling them (or, as Prof. Henry Draper would call them, the spaces between the bright lines), were narrowed, whereas the interspaces were broad and of a perfectly even tint throughout, coming up to a sharp edge. That is the difficulty which I felt in accepting the view that those are really bright lines. I have not seen Dr. Draper's photograph; but with regard to the question of matching, I should like to ask Mr. Ranyard, as he attaches so much importance to the coincidence of 4 lines, how much he would attach to the coincidence of 65?—because I suppose he is speaking of Prof. Henry Draper here; but

Prof. J. C. Draper has found a coincidence of 65 dark lines of oxygen [*], so that we are in a difficulty at present as to whether there are 4 bright lines of oxygen or 65 dark lines. So far as the evidence which has been put before me goes, I prefer to take Prof. J. C. Draper's 65 lines, which have been mapped on a very much larger scale, so that we can determine the coincidences with greater accuracy. Prof. Henry Draper may have determined them with great accuracy; but the one photograph which was exhibited was on far too small a scale to settle a nice point of that kind with any degree of certainty whatever. The probable error of the determination of coincidence is far too large to settle a question like that. In the negative the lines may have been much better defined, but still the matter of the coincidence of those bright lines in the case of comparison spectra is very difficult indeed. I can speak from experience in comparing the star spectra; and in comparing the bright magnesium lines with the *b* lines, the slightest displacement of the spectroscope or of the electrodes will alter and shift the lines, so that it is very difficult to answer for minute quantities in those coincidences.

Mr. Ranyard. That makes the matching all the more important when they do match over a large area. Mr. Christie has construed what Prof. J. C. Draper has done as though it was a contradiction of the work of Dr. Henry Draper, whom we all know. I do not at present know who Prof. J. C. Draper is.

Mr. Schuster. He is his elder brother.

Mr. Ranyard. But it is not a contradiction. We cannot know how many spectra oxygen may have. Oxygen may have allotropic forms; in fact we know in the laboratories it has allotropic forms. It may have several spectra, and it is not impossible that we may find a dark-line spectrum in the Sun at one depth, and a bright-line spectrum at another depth. So far as I read the paper of Prof. J. C. Draper, he made a photograph of the dark lines of the Sun, and he found the coincidences which have not been found by other men. His method appeared to me to be faulty; he enlarged the photograph on a screen by a magic-lantern process, in which there would be great distortion. I should think it a very unsatisfactory method of finding the positions of lines to throw them upon a screen and then to determine their positions. He did not measure the oxygen lines himself; in fact I take it that there is nobody who has measured those lines and published them, besides Dr. Henry Draper, M. Thalén, M. Plücker, and Mr. Huggins. He took the position of the oxygen lines from Watts's 'Index of Spectra.'

Mr. Christie. I do not think that is quite a correct statement; he determined his scale from the iron lines.

Mr. Schuster. He suddenly changed his method of observation. Up to a certain point he took the iron lines, and from these he took the other method, the oxygen lines themselves. I find that when he took the iron lines, they agree, as well as one could possibly expect, with my own measurements, which have been taken by similar but still different methods. When he took the oxygen lines themselves as reference lines, his wave-lengths do not any more agree with mine, and I believe them to be wrong; but that only applies to about one fourth of the whole number. The other measurements agree very well, and I think they have been fairly taken; but I think he has underrated the probable error of one determination. It is probably a very large fraction of the distance of two dark lines in the solar spectrum; and for this reason I did not think his paper is conclusive as to the existence of corresponding dark lines.

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[*] 'Observatory,' No. 19, p. 232.

Mr. Ranyard. There is a difference of two or three tenth-metres between him and Dr. Huggins.

Mr. Christie. I am aware of that; I only mention it to show the difficulty in the comparison. If we take the 4, I do not see why we should exclude the 65. Not one of those lines which Dr. Rutherfurd's and our photographs show is shown upon this photograph of Dr. H. Draper's; it shows no more than is shown in the paper print.

Mr. Ranyard. I think it shows a great many more than one in the print. This was taken with a dispersion equal to four prisms.

Mr. Christie. But that is the point at issue. Can you decide the question with a dispersion of four prisms?

Mr. Ranyard. We cannot say with absolute certainty that iron is in the Sun; but the probability that iron is there is enormous, because there is a very large number of iron lines, and the coincidence of those lines has been determined with a large dispersion; but nobody in England has brought the spectrum of oxygen by the side of the solar spectrum and photographed it; nobody has, indeed, had an instrument with which he would have been able to produce the oxygen spectrum sufficiently bright and continuous; and until photography shows that there are not those coincidences, I hold there is a large probability that such coincidences do exist.

Mr. Christie. Mr. Ranyard began by stating that Prof. Henry Draper's photograph showed the same fine lines which I had instanced as tending to show that the spaces in the solar spectrum could not be bright lines of oxygen. Now it appears that the lines of which Mr. Ranyard is speaking are totally different lines; they are spaces in another part of the spectrum, and they are not broken up at all in Prof. H. Draper's photograph. The whole point of the matter then is that in Prof. H. Draper's photograph there are certain spaces in which the definition is not sufficient to show fine dark lines. In Dr. Rutherfurd's photograph, and in those taken at Greenwich, there are certain fine dark lines shown in these spaces; and the question is, how to explain the presence of those fine dark lines in the middle of the so-called bright oxygen lines. As I understand Mr. Ranyard, he now withdraws his statement that fine lines are shown in those spaces which I had specified.

Mr. Ranyard. I do not withdraw the statement that those lines appear a great deal more broken up by further dispersion. I have here a little very rough map of a group of lines in the part of the spectrum drawn by Mr. Christie, showing a great many more lines than are given in Mr. Christie's drawing; it was made with a dispersion corresponding to eight prisms of 60°, and yet I saw a great many more lines than are shown in the drawing which Mr. Christie says corresponds to a dispersion with his half-prism spectroscope of 60 prisms*.

Mr. Christie. No, I said 15 prisms.

Mr. Ranyard. With eight prisms I saw a great many more lines; but that has nothing to do with the way in which the oxygen spectrum may be broken up. You have not, and nobody else has, so far as I understand, examined the oxygen spectrum with a high dispersion, and until that has been done, and it has been shown that there is no coincidence, the matter stands as it was.

Mr. Brett. We have all heard with great pleasure that one of our Secretaries has been so fortunate as to make a very important photograph of the eclipse in America, and the Meeting will be very much disappointed if his natural modesty should be allowed to stand between us and the gratification we anticipated at seeing this valuable photograph. May I suggest that it is probably on the table and should be shown to us.

Mr. Ranyard. It is not on the table.

The President. I think with a valuable negative like that it is not desirable to have it on the table†.

Mr. Marth said he would call attention to a phenomenon occurring next year, which, though it could not be observed by astronomers on the Earth, possessed perhaps as much interest as, say, a total eclipse of the Sun observable only at some inaccessible region near one of the poles. The phenomenon in question would only be visible to astronomers on the planet Mars. On Nov. 12, 1879, before 2^h G.M.T., a small black body would make its appearance on the south-following side of the disk of the Sun; in 6 minutes it would have fully entered upon the disk and would proceed slowly from left to right. About a quarter past 4^h, another and bigger black body would encroach upon the disk and would occupy 21 minutes before it had fully entered upon it. The two bodies were the Moon and the Earth, and they would be visible from all parts of Mars where the Sun was above the horizon. But observers placed along a certain zone or track would have the opportunity of seeing a third and apparently much bigger body cross the Sun's disk. It was at present somewhat uncertain at what exact time this third body would make its appearance, probably a quarter to 4^h, while the moon was yet alone on the disk. This third body would come from the right-hand side in a direction at a considerable slant towards the south; and Martial observers would have to look sharp to observe all the contacts, since the time for doing so would be limited to some twenty or thirty seconds. This third body was Phobos, the inner satellite. But Martial astronomers would be much more interested in the Transit of the Earth and Moon than in that of Phobos, since in the course of a Martial year there were no less than about 1388 transits of Phobos across the Sun visible from some part or another af the planet, while the number of the transits of Deimos was about 133. On the other hand, the Transits of the Earth and Moon were rare occurrences, the last one having taken place in 1800, while the next to follow would happen in 1905. About a quarter to 10^h the Moon, which meanwhile had drawn nearer to the Earth, would quit the Sun's disk and the last external contact of the Earth with the disk would take place at Greenwich midnight. But before internal contact, about half past 11^h, Phobos would again make its appearance upon the Sun for certain stations, after having meanwhile performed a whole revolution round the planet.

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* [Mr. Ranyard's map of the spectrum was not produced at the Meeting. —Ed.]

† [Mr. Ranyard's eclipse photograph was not produced.—Ed.]

The following papers were announced:—

T. H. Safford. Note upon the Proper Motion of 5 Serpentis.

A. T. Arcimis. Transit of Mercury, 6th of May, 1878, observed at Cadiz.

J. Tebbutt. Discovery of Encke's Comet.

Maxwell Hall. Additions to the Theory of the Sidereal System.

Prof. C. Pritchard. Account of additional buildings and proceedings at the Oxford University Observatory.

Prof. C. Pritchard. Observations of the periodic Comet of Tempel, 1878.

Prof. H. Draper. The Solar Eclipse of July 29th, 1878.

T. H. Safford. Note upon the Star Bradley 2935.

E. H. Liveing. On a portable Star-finder for altitude and azimuth telescopes.

The following were ballotted for and duly elected Fellows:—

B. G. Jenkins and Rev. J. M. Coates.

The following were duly elected Associates:—

Dr. C. Bruhns, Baron H. von Dembowski, G. W. Hill, and Prof. E. Schönfeld.

The Sun's Corona during the Eclipse of 1878.

We have arrived at a stage of eclipse observation where regular work may be said to begin. Hitherto a general survey only was taken of the various phenomena, and hardly any quantitative measures were made. This may be compared to the preliminary work of any scientific investigation. The most important results, however, will only come out by a study of minute changes in different parts of the corona and during different eclipses. Some of the American observers during the present eclipse (as Prof. Eastman in his spectroscopic observations, and Prof. Arthur Wright in his polariscopic observations) have been alive to this fact, and may be said to have inaugurated a new era. In future the results will altogether depend on such numerical measurements. I shall give in the following pages what seems to me to be a fair statement of the results obtained during the last eclipse, and of their bearing on other eclipses. We must remember, however, that the Washington report has not appeared yet, and that although some of the results of the Washington observers are known, others, perhaps equally important, may appear only in the detailed report.

To begin with the spectroscopic observations. These have been partly photographic, and partly made by eye observations. Dr. Draper and Mr. Lockyer obtained photographs by means of slitless spectroscopes. These slitless spectroscopes, or prismatic cameras, as they have been termed, are useful chiefly in determining the nature of the light; but they also give further valuable information. In the eclipse of 1875, in Siam, it was possible to decide, for instance, to what height in the corona and how far into the ultra-violet the continuous spectrum left a photographic impression on the plate during the time of exposure (see Phil. Trans. 1878, part i.). At that time a distinct ring was observed round one part of the sun, corresponding to the hydrogen line G; this year only a continuous spectrum appeared. Dr. Draper's remarks on the subject are as follows:—"It was the general impression that it would be impossible to obtain a photograph of the diffraction-spectrum of the corona; but the result proved the contrary, for on developing the photograph immediately after totality it appeared that I had obtained a spectrum without the least trace of a ring, showing that the corona on this occasion did not consist of an incandescent gas giving a few bright lines, but that its light was due to reflection of sunlight by solid or liquid bodies surrounding the sun like a cloud of meteors."

The result thus obtained by the photographic method is in entire accordance with that obtained by eye observations. I shall quote Professor Young's words on the subject. His experience in spectroscopic work during eclipses renders his opinion decisive. He says, in a letter to the 'New York Times' (reprinted in 'Silliman's Journal' for September*:—"While, however, there may be room to question the conclusion that the corona this year was uncommonly faint, there can be no question that its spectrum was profoundly modified.

"The bright lines which come from its gaseous constituents were conspicuous in 1869, and in all the subsequent eclipses until the present one; but this year they were so faint as to be seen by only a few of the observers, while the great majority missed them entirely, seeing only a continuous spectrum.

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* *Cf.* 'Observatory,' No. 19, pp. 234, 235.

".... The same constituents appear in the corona as hitherto only in altered proportions, as might have been and was expected by students of solar physics. In 1869, 1870, and 1871 the gaseous elements of the corona—the hydrogen and '1474 stuff,' whatever that may be—were in such quantity and condition, and rose so high above the solar surface, that their lines were conspicuous in the coronal spectrum, and attracted the attention of observers far more forcibly than the feeble continuous spectrum of the light emitted from, and reflected by, the minute solid and liquid particles which also constitute an essential element of the corona; at present the condition is reversed. The gases are either too small in quantity or too cool to be conspicuous. The lesson, and it is an important one, is simply, as has been said, that to a certain extent the corona sympathises with the sunspots."

Professor Eastman also testifies to the great faintness of the green line, and I can vouch for the fact from my own observation. But, although Prof. Eastman agrees with every other observer that the green line was faint, he had no difficulty in finding it and tracing it round the solar disk, noting down, with the assistance of Mr. Pritchett, at what height from the Sun's limb the continuous spectrum and green line disappeared. The result was rather remarkable; for although the corona extended to a much further distance in a direction approximately parallel to the solar equator, the visible spectrum disappeared at distances sensibly equal in all directions. The value of Prof. Eastman's measurement lies chiefly in the fact that it will afford us a ready means of comparison between future eclipses and the present one.

Whether the great faintness of the green line during the present eclipse is really due to absence of sun-spots, is at present impossible to decide with certainty; but, as Prof. Young remarked, it is a fact which "might have been and was expected by students of solar physics."

Another interesting matter was settled during the present eclipse. It was found in 1875 that the most actinic rays of the prominences were not due to hydrogen, but to a line near H; that is to say, that in photographing a prominence we do not photograph the hydrogen at all, but some other substance. Prof. Young, who had seen at Sherman the H lines reaching high up in the chromosphere, suggested that the line near H was H itself, a question which the small dispersive power of the prismatic camera used in Siam did not allow us to decide. During the late eclipse Prof. Young, using a fluorescent eyepiece looked for and discovered the H lines reversed in the chromosphere as the Sun reappeared. The question whether magnesium or calcium reaches up highest in the Sun's corona has therefore received a satisfactory settlement.

It is fortunate that Profs. Morton and Barker, observing in Dr. Henry Draper's party, saw the Fraunhofer lines reversed in the light of the corona. The reversal escaped several other observers; yet it undoubtedly exists, and has been proved to exist also during former eclipses. It would be rash, however, to assume that the whole of the continuous spectrum is reflected sunlight. Any particle which is sufficiently large to scatter sunlight will give a continuous spectrum if it is luminous, and luminous it must be so near the sun. The polariscope will most likely settle the proportion of the scattered light present.

Professor Arthur Wright has taken a series of very careful measurements of the amount of polarization at different distances from the Sun, and has confirmed his result by three independent methods. He found that for three different distances, the nearest of which was about 5′ away from the Sun, the polarization rapidly decreased as the distance increased. The result is remarkáble, for I have calculated that whatever hypothesis we make as to the distribution of scattering matter in the corona, the polarization ought always to increase as we move away from the Sun. I see only one way out of the difficulty, and that is to suppose that at some distance away from the Sun the light is that scattered from matter too large to polarize the light; that this matter is broken up as it falls into the Sun; so that gradually as we approach the Sun the light reflected from large particles decreases, and that scattered from small ones increases. Very near the Sun, the observations made by Prazmowski (1860), Janssen (1871), and Winter (1871) are in accordance with theory, and give a decrease of polarization as we approach the Sun. I need not insist on the importance of making accurate polariscopic measurements during future eclipses so as to discover any change in the quantity of scattering matter.

Many valuable photographs have been taken during the late eclipse; the most perfect ones, however, were two sets taken by the Washington observers: one set was taken by Mr. Rodgers in Prof. Hall's party, La Junta, Colorado; the other by Prof. Harkness in Wyoming. The two sets are so much alike that were it not for a slightly greater extension in one of the sets, owing, most likely, to the greater transparency of the atmosphere in Colorado, it would be impossible to decide which was which supposing they were mixed together. It will be interesting to compare the details of these photographs with those shown on Mr. Penrose's excellent drawing.

In conclusion, I wish to mention a speculation which has forced itself on me, and which, however wild it may at present appear, deserves at any rate to be disproved before it is set aside as useless. So much ridicule is thrown in this country on any one who attempts to find out something more about sun-spots than is already known, that only a full sense of its importance can induce one to mention the subject. Yet even those who have the most intense hatred of sun-spots and every thing connected with them, admit, I believe, that sun-spots exist, and some of them go even so far as to believe that the existence of an eleven years' period is a fit subject for discussion. This period must be produced by some cause either internal or external to the Sun. An internal cause is extremely unlikely, and an external cause has not yet been discovered. Now during the discussion of the late eclipse, the view that the corona is at any rate partly due to meteor-streams circulating round the Sun has again come prominently forward. In their approach towards the Sun the increased chances of collision amongst the meteors and the disintegration of matter owing to the great heat, must cause many of them to fall into the Sun. The inequality of temperature produced by such a fall on the surface of the Sun may cause currents, which, under certain circumstances, will produce cyclones, which we observe as sun-spots. Is it not possible that these meteor-streams have an eleven years' period; that every eleven years more matter falls into the Sun and more sun-spots are produced? If the corona is partly produced by meteor-streams, we can hardly escape the conclusion that these meteor-streams are periodic; for on looking at the shape of the corona, as indicated in the drawings of 1868, 1869, 1870, 1871, 1874, 1875, and 1878, we perceive a gradual series of changes, until during the last few years the corona reverts approximately to the shape of 1868. That the corona varies infinitely in detail is certain, but it is equally certain that its general rough outline, though changing, is regulated by fixed laws. Look, for instance, at the drawing of the eclipse in 1868 (copied in Lockyer's 'Solar Physics,' p. 289); we see short polar rifts and long equatoreal rifts forming what the Siamese called fishtails in 1875. In 1869, when sun-spots had considerably increased, the symmetry round the Sun's axis is almost entirely gone, the polar rifts having as great an extension as the equatoreal ones. In 1870, when sun-spots were near a maximum, it is not possible to distinguish any symmetry on Mr. Brothers's photograph. In 1870, when sun-spots had decreased again, the polar extension is still large, yet an axis of symmetry can be distinctly traced on Col. Tennant's photographs; in 1874 the corona has almost regained the shape of 1868, and the general outline has remained sensibly the same during the long period of minimum through which we are at present passing. This series of changes may be accidental, yet it is highly suggestive.

ARTHUR SCHUSTER.

The Stars in their Courses.*

ONE of the commonest defects in star-maps is the confusion caused by the presence of lines and marks which have no existence in the heavens. Circles of R.A. and Dec. and the names of constellations and stars are necessary for purposes of identification; but they are apt sadly to interfere with the recognition of the stellar configurations, being usually far more conspicuous than the stars as represented. Out of mere wantoness on the part of their makers, as it would seem, star-charts used to be, and globes even now are, disfigured by the figures of men and animals, which effectually distract the eye from such faint traces of the host of heaven as might painfully be made out amongst the multiplicity of lines, circles, names, and letters. Argelander is almost alone in having paid attention to this important practical point; and in his 'Uranometria Nova' he was careful to subordinate the reference-lines and marks by having them engraved as lightly as possible, and in red, so that at night they are scarcely noticeable. His large Atlas and that of Dien, on somewhat the same model, are equally successful in this respect; and, with the exception of the present work, they are the only maps, as far as we know, in which the configuration of the stars can be traced with comfort.

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* 'The Stars in their Courses.' By Thomas Sebastian Bazley, M.A. Trübner: London, 1878.

Mr. Bazley has evaded the difficulty to which we have alluded by having a duplicate set of maps, one of which shows the stars without circles of R.A. and Dec. or distinguishing marks, whilst the other has the reference-lines and names inserted, so that identification is rendered easy. The stars are represented as black dots on a white ground, and the principal constellations, such as Orion, Pegasus, Ursa Major, Cassiopeia, Leo, the Hyades, &c., strike the eye at once. The brightest stars, Sirius, Arcturus, Vega, and Capella, are, however, not sufficiently conspicuous, Mr. Bazley having used the same size of dot for them as for stars of 1½ mag. The configuration of stars in Boötis is quite spoilt by the near equality in size of Arcturus and ε Boötis on Mr. Bazley's maps; but this is almost the only instance of the kind we have noticed. It is, in any case, a more pardonable fault to make some of the dots relatively too small than to make all the bright stars more than twice the size of the Moon, as Mr. Proctor does in his maps, thereby destroying all trace of resemblance to the objects intended to be represented. Even in Mr. Bazley's maps the stars of the second, third, and fourth magnitudes might rather be reduced in size than those of the first magnitude increased. However, considering the practical difficulties in lithographing the maps, the resemblance to the heavens is as close as can reasonably be expected.

Another feature, to which we cannot give the same unqualified commendation, is the representation on one map of the whole of the heavens visible to an observer in our latitude. This system has its advantages and its drawbacks. To take the latter first: if an attempt be made to include all stars which at any time appear above our horizon, the pole must be made the centre and the map must extend to N.P.D. 128°. Whatever projection be adopted, there must be great distortion. To diminish this, Mr. Bazley adopts the alternative plan of putting the zenith in the centre, and merely giving those stars which are above the horizon at the hour of sidereal time chosen for the particular map. Thus the radius of the map will be 90° instead of 128°, the boundary being in this case the horizon, and the distortion will be comparatively small. Its effect will be to convert a square into a lozenge-shaped figure, when far east or west of the meridian, as may be well traced on

Mr. Bazley's maps in the case of the square of Pegasus and Orion. But it is obvious that a number of maps will be required to represent the appearance of the heavens at different hours; and the question arises whether, with the twelve maps which Mr. Bazley gives (in duplicate), more could not be done if each map only included a portion of the heavens. A larger scale might, of course, be adopted, and the fainter stars might be inserted. But it must be admitted that Mr. Bazley's plan has great advantages. A glance at the map corresponding to any particular hour shows what constellations are above the horizon and in what positions; and it thus forms one of the best companions to the telescope. Further, there is no break of continuity in the series of constellations. If each map represent only a portion of the heavens, it is necessary to allow a very liberal overlapping—which, by the way, is never done in practice,—or to adopt some such projection as Mercator's, which will allow of the representation of the greater part of the heavens on one continuous sheet, with separate charts of the stars near the north and south poles. By extending these latter so as to overlap the central zone considerably, the configurations of the stars could be followed without interruption; but maps on such a system would fail to show the positions of the various constellations in the visible heavens at any time, which is one of the great merits of Mr. Bazley's charts. A set of circles of altitude and radii of azimuth, lithographed (in triplicate) on tracing-paper, adds to their usefulness in this respect.

On the maps, which extend to 36° S. Declination, the places of 1400 stars (down to 5½ mag.) are laid down on the equidistant projection for the epoch 1890, and in the Introduction the R.A. and Dec. are given for 1558 stars, together with their magnitudes. The collation of the various magnitudes assigned would have been more useful if Mr. Bazley had confined himself to original authorities. The British Association Catalogue is notoriously untrustworthy in this respect; and it is hard to conceive how any one could take Mr. Proctor's maps as evidence of variability in a star. They would be more likely to give evidence of human fallibility. On the other hand, a comparison of Piazzi's magnitudes would have been instructive, as he paid special attention to this point, and his observations were made at a distant epoch. Again, Sir J. Herschel's photometric determinations of the magnitudes of 150 stars, though he only went as far as the 3½ mag., might, with advantage, have been given. Herschel devoted much time to this subject, and his results appear to deserve every confidence, whilst, to the extent to which he went, his magnitudes are not sensibly affected by the peculiar scale he adopted.

On the whole, we think that Mr. Bazley's star-maps are the most convenient for use that we have yet seen. They give all the stars seen under ordinary circumstances with the naked eye; and for those who require more, they form an excellent key to Argelander's or Dien's charts, indicating, as they do, where the principal stars may be found in the sky at any hour of the night. EDITOR.

Double Stars for December.

[*The colours of* γ *Andromedæ.*—On this subject the following observations will be read with interest by Mr. Barneby if he has not already met with them.

Dawes (Mem. R. A. S. Vol. xxxv.) has these descriptions of the colours of the close pairs:—"Both *blue*;" "both *greenish blue*;" "both *green*;" "both *blue*;" "both *blue*;" "both *pale green*;" "A *green*, B *deeper green*." When these estimates were made the stars were never seen clearly separated. When the separation was effected, the following descriptions are recorded:—"B very *pale yellow*, C *blue*;" "B *greenish yellow*, C *bluish green*." So that Mr. Barneby's careful observations are clearly confirmed by the estimates of Dawes. On the other hand, Dr. Dunér, a most careful observer of large experience, and using a fine 9·6-inch refractor, *always* sees B *blue* or *green*, never *yellow*, and C always *blue*. His observations were all made when the separation was certain or considerable. The colours of B C given in 'The Observatory' for October were taken from M. Flammarion's extensive Catalogue of Double Stars.]

The following pairs have been but little observed, and therefore call for some attention at the present time.

Σ 269. R.A. $2^h\ 21^m{\cdot}1$, Dec. +29° 20′.

Magnitudes, A 7·5, B 9·8 (Σ); A 6½, B 10 (Sm.); A 8, B 9½ (Mo.). Colours, A yellow, B ash.

1873·9	345°·9	1″·4	Wilson and Seabroke.

Σ 295. R.A. $2^h\ 35^m{\cdot}1$, Dec. −1° 12′.

Mag. 6, 9·2. Colours, A yellow, B ash. Binary. Common proper motion −0″·003 in R.A. and +0″·053 in N.P.D.

1873·94	325°	4″·7	Gledhill.

Σ 293. R.A. $2^h\ 35^m{\cdot}5$, Dec. +56° 33′.

Mag. 8·5, 11·7.

1867·62	B invisible	Dembowski.

Σ 296. R.A. $2^h\ 35^m{\cdot}9$, Dec. +48° 43′.

Mag. 4·2, 10. Colours, A yellow. A B binary. Rapid common proper motion. Smyth gives A 4, B 13, C 11.

		°	″	
A B	1873·9	296	16·5	Wilson and Seabroke.
A C	33·6	219	27·0	Smyth.

Σ 299. R.A 2^{h} $37^{m}{\cdot}1$, Dec. +2° 46′.

Mag. 3, 6·8. Colours, A yellowish, B ash. Binary. Proper motion $-0^{s}{\cdot}011$ in R.A. and $+0''{\cdot}19$ in N.P.D.

1877·08 291° $2''{\cdot}82$ Spörer.

Σ 305. R.A. 2^{h} $40^{m}{\cdot}6$, Dec. +18° 52′.

Mag. 7·3, 8·2. Colours, yellow. Motion rectilinear.

1877·1 319° $2''{\cdot}72$ Spörer.

JOSEPH GLEDHILL.

Meteor Notes for December.

OBSERVATIONS may be made before sunrise on the first few days, and renewed after the 19th. In the evenings the period from the 14th to the 25th will be free from moonlight, and suitable for this work. A few *Andromedes* are sometimes visible on about the 6th. In 1872 the Italian observers traced eleven good meteors of this shower on the 5th–7th, and Greg found (from the British-Association observations, 1867–74) it confirmed at 25° +42° for the 4th–8th (see No. 172, A_{17} of his catalogue). The *Taurids* II. (80° +23°, max. 6th) and Geminids (105° +32°, 9th–12th) cannot be well seen this year, owing to moonlight. A very decided shower of bright streak-leaving meteors from 145° +7°, noted on the early morning of the 9th, 1877, will hardly merit observation, from the same cause. There is another prominent radiant at 209° +67° (near α Draconis) to be expected during this month. On the 10th–12th in 1873 Clark found also an active shower at 57° +6°, which agrees very well with one of Schmidt's positions at 55° +5°, Dec. 10–21. A morning display from near β Böotis was detected for the first time in December on the 10th–13th, 1877, at 221° +43°, though it is a point from which many shooting-stars are directed in January; for in 1869, Jan. 20, A.M., Zezioli recorded numerous small meteors from it, and in 1877, Jan. 10, A.M., the writer traced a shower of very swift meteors having the same centre. In and around Ursa Major there are many visible radiants at this epoch, and they should be looked for whenever a clear sky and absent Moon allow. Fireballs have frequently been seen during the period from the 9th to the 12th, and the 26th is another good date for such observations.

Recent Observations.—At Bristol the *Orionids* were watched for on Oct. 22 between 11^{h} 30^{m} and 13^{h}. 34 meteors were counted, of which 11, or one third, belonged to the radiant near ν Orionis. They were small and short, with streaks and rapid paths. Nothing was seen of any *Leonids* on Nov. 13, the sky being overcast.

Mr. H. Corder, of Chelmsford, registered a large number of meteors between October 22 and the end of that month. In addition to the *Orionids* and several simultaneous showers from Gemini (mentioned by him in another column), he determined radiants in

Ursa and in the region about Aries. 17 slow meteors gave a centre at 37° +10° (μ Ceti), and 13 others were projected from a radiant at 32° +34°. There was also a group of feeble showers from points between Aries and Taurus giving an average centre at 43° +15° (15 meteors). The *Gemellids* were represented by 8 meteors intersecting at 106° +27°. On October 27 a few paths were noticed from a radiant at 130° +54°, and on Oct. 22 and ensuing nights feeble showers came into play at 160° +57°, 160° +37°, 290° +78°, 92° −7°, 96° +3°, and 325° +37°. A stationary meteor at the latter point was visible on October 25.

Mr. V. Cornish, of Debenham, Suffolk, saw several meteors during an hour's watch on Oct. 22, 8^h to 9^h, indicating a radiant at 335° +31°. This is probably the same as Mr. Corder's at 325° +37°, and they confirm one of Heis's autumnal showers (U_2), 328° +35°, Oct. 17–31 (18 meteors).

Mr. E. F. Sawyer, of Boston, Mass., observed a very active shower of *Lyrids* between Aug. 23–Sept. 1, the radiant-point of which was very accurately deduced as at 282° +42°. No less than twenty meteors were mapped belonging to this stream. They were quite bright and quick, with long paths. Mr. S. C. Chandler, Jun., at Marlboro', N.H., also recorded a few meteors belonging to this shower, which was detected some years ago by Mr. Greg, who placed the radiant at 285° +44° for the period Aug. 2–Sept. 25.

In our "Notes" for October many shooting-stars were described as having been observed by the writer on the night of Sept. 1 last. The date seems a special one for large meteors. On Sept. 1, 1873, Mr. F. J. Clark at Ackworth, Yorkshire, saw "quite an abundance of bright meteors. Between 11 and 12 P.M. nine were seen, including some very fine ones, mostly in the south." Sept. 1, 1854, Heis, at Münster, saw many bright meteors. Sept. 1, 1866, he also found them moderately numerous. Aug. 31, 1867, Zezioli saw many meteors, and mentions that in the early part of the night one of the chief radiants was in Cygnus. Sept. 1, 1869, at the Radcliffe Observatory, Oxford, six small meteors were visible (11^h 38^m to 12^h), "shooting from points between α Lyræ and α Ophiuchi towards the latter star." Sept. 1, 1871, two fireballs were seen in England. Sept. 2, 1872, a "magnificent bolide" observed by Serpieri at Urbino, Italy. Sept. 1, 1874, the largest fireball seen in England during the year. Sept. 2, 1875, large and brilliant meteor observed at London. Sept. 1, 1877 and 1878, large meteors seen at Bristol. These facts amply prove the date is one of more than ordinary note, and with the view to determine the chief radiant-point a projection of the meteor-paths recorded on this night by many observers has just been completed. By far the best shower is one about 10° north of α Cygni and near ψ, at 306° +54°, to which thirty-one meteors belonged, fifteen of them very brilliant. This, therefore, may be regarded as a strong shower, with max. on Sept. 1; and it is remarkable that it almost coincides with the radiant of the fireball of Sept. 3, 1875, which Capt. Tupman placed at 311° +52°.

W. F. DENNING.

CORRESPONDENCE.

To the Editor of 'The Observatory.'

The October Meteors.

SIR,—

This year is an unfortunate one for the observation of the principal recurring meteor-showers, as there has been, and will be, a bright moon at the time of maximum activity.

The Orionids have been no exception to this rule, and it was not until Oct. 22nd that I had any opportunity of observing them, as, to make matters worse, the weather was extremely cloudy when the display should have been at its height.

Commencing a watch at 6.30 on that date, I found meteors unusually scarce, only 13 being seen up till 10 o'clock, none of them being Orionids. After that hour matters mended considerably, 17 being recorded between 10 and 11; 16 between 11 and 12; 13 between 12 and 1; 26 between 1 and 2, and at the rate of 29 per hour for the last part of the watch, and I much regret not having continued it longer.

Of these 99 meteors, 35 were of the Orionid group, 25 leaving streaks which were generally bright.

After this date they were not so numerous, but altogether in October I recorded 52 from the position given in Mr. Greg's map.

By my maps, however, I found three fairly distinct radiant-centres. One at 90° +14°, at ν Orionis, the true point; another well marked at 88° +24° near η Geminorum; and a third, more doubtful, at 96° +20° near ν Geminorum.

Later in the month a fresh radiant was noted at 102° +19° between ξ and γ Geminorum (16 meteors); these were similar to the Orionids, quick, with a great proportion streak-leaving, the streaks in some cases decidedly brightening after the extinction of the nucleus.

The very few first magnitudes seen were yellow; but, as a rule, the meteors were very free from colonr.

Although the Perseids are very noticeable for the number of large meteors, the Orionids far surpass them in the number of streaks in the lower magnitudes, the proportion being more than double.

These were the principal showers noted in October; but I also reobserved the slow and small quick meteors with radiants in Aries and Taurus, the Gemellids, and a shower from Triangulum. The true Taurids had just commenced when the moon again rendered observation almost useless.

The great difference in the appearance of meteors has led me to divide them roughly into three classes:—first, the swift arrowy meteors, generally streak-leaving, with radiants in the east and north-east; type "Orionids;" second, medium or common-place meteors, short, small, quick, and almost colourless; type "Geminids:" and third, the slow meteors, generally orange with trains; type "Taurids:" and I have made the following deductions from my observations during the last two years. The smaller numbers of the two latter classes are owing to the greater rarity of the slow ones and the uncertain radiant-points of the common-place ones.

	Total.	Streaks.	1st, Str.	2nd, Str.	3rd, Str.	4th, Str.	5th, Str.
Swift meteors...	400	178	44, 35	99, 73	97, 44	121, 24	39, 2
Medium	200	13	4, 1	14, 3	60, 7	97, 2	25, 0
Slow (trained)...	150	26	5, 5	19, 12	45, 7	59, 2	22, 0
Totals	750	217	53, 41	132, 88	202, 58	277, 28	86, 2
Per cent. :—							
Swift meteors...	100	44	11, 79	25, 73	24, 45	30, 19	10, 5
Medium	100	6·5	2, 25	7, 21	30, 11·5	48·5, 2	12·5, 0
Slow (trained)...	100	17	3, 100	13, 63	31, 18	41, 3·5	13, 0
Average	100	22·5	5, 68	15, 52	28, 25	40, 8	12, 1·7

I am, Sir, yours truly,
H. Corder.

Writtle, Chelmsford,
1878, Nov. 7.

New Red Star.

Sir,—

On several occasions lately I have remarked the star 16 (Fl) Aurigæ to be strongly reddish. It is not included in Birmingham's 'Catalogue of Red Stars.' It was rated 5th mag. by Heis.

I am, Sir, yours faithfully,
J. E. Gore.

Dromard, Co. Sligo,
1878, Nov. 16.

NOTES.

Absorption in the Ultra-Violet part of the Spectrum.—The following notice of experiments and observations by M. Soret may prove of interest:—M. Soret's researches, confirming and continuing the work of Professors Stokes and Miller, have been carried on in the ultra-violet part of the spectrum by the aid of fluorescence; their general scope being (1) to determine the general absorptive power of various liquids or of solids in solution; (2) to determine the character and positions of the absorption-bands of various spectra. Details of instrumental appliances, and of the general absorptions and absorption-bands in the liquids and solutions experimented upon, will be found in M. Soret's memoirs*.

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* Archives des Sciences Physiques et Naturelles, No. 243, Mars 1878, et No. 248, Août 1878. Recherches sur l'Absorption des Rayons ultra-violets par diverses substances. Par M. J. L. Soret.

As a summary it may be stated that the observations extend somewhat beyond the most refrangible line (yet observed) of aluminium, *i. e.* far above U. The thickness of the liquids and solutions usually employed was under 1 centimetre. Various acids and chlorides, nitrates and sulphates were experimented upon; special studies were made of the absorption-spectra of chromates, of nitrates, and of nitric acid.

In his second memoir M. Soret examines the absorption-spectra of the earths forming "gadolinite" and of didymium. He comes to the conclusion that "gadolinite" consists of yttria, erbia, terbia, and some other base.

Memoir I. contains some useful practical remarks on the influences of thickness and concentration of solutions under examination upon their various spectra, and also a record of experiments as to the relative transparency of quartz and Iceland spar for the ultra-violet part of the spectrum. These experiments show that the transparency of Iceland spar does not extend so far in the ultra-violet as does that of quartz.

M. L. H.

THE TRANSIT OF MERCURY.—In a paper read before the Albany Institute Mr. Lewis Boss, the director of the Dudley Observatory, has discussed a large number of observations of contacts, and deduced the corrections to the American Ephemeris and to the British Nautical Almanac respectively. In the former, Le Verrier's old theory of Mercury is used, and in the latter his later tables, and the importance of the comparison lies in the circumstance that these latter include a term due to the supposed attraction of an intra-Mercurial planet. The mean corrections to the predicted times of contact resulting from the observations are as follows:—

			s		s
First contact,	6 obs.	Corr. to Amer. Eph.	− 45·7	to N. A.	− 4
Second contact,	15	"	− 61·8	"	−20
Third "	11	"	−124·4	"	−18
Fourth "	10	"	−141·7	"	−35
		Means......	−93·4		−19·5

Thus it appears that the later tables, with the term due to an intra-Mercurial planet, give a satisfactory representation of the fact. The apparent corrections to the N. A. range from -10^{s} to -1^{s} for first contact; -32^{s} to -10^{s} for second contact; -26^{s} to -4^{s} for third contact; and -41^{s} to -24^{s} for last contact. Taking simply the discordances from the mean in each case in the tables given by Mr. Boss, the mean error of an observation is 2^{s} for first contact, 6^{s} for second, and 5^{s} each for third and fourth. Considering the small number of observations of first contact, but little importance can be attached to the estimate of mean error in this case, and we should not be far wrong in taking the mean error as about the same for all four contacts, and equal to 5^{s}. It should be mentioned that Mr. Boss, in his computations from the American Ephemeris, used for the diameter of the Sun the value given in the 'Berliner Jahrbuch,' 16′ 1″·2, and that with the 'Nautical Almanac' Le Verrier's diameter is used.

Micrometrical measures of the diameter of Mercury during the transit gave 11″·30±0″·14 uncorrected for irradiation or possible expansion of the screw. This value would give 176^{s}·6 as the interval between external and internal contact; the observed interval was 172^{s}·4 between first and second, and 171^{s}·2 between third and fourth.

Mr. Boss observed the second contact, using the 13-inch equatoreal, with a power of 275, and a solar diagonal, the definition being very bad. He attaches great importance to the use of a high power, and intended, under good atmospheric conditions, to employ a power of 500. His assistant, Mr. Landreth, was sent with a party to observe the transit at Helderberg, 15 miles from Albany and 1600 feet above the Observatory; and at this elevated station he observed the contacts under most favourable circumstances. Mr. Landreth, who observed with a 3·9-inch comet-seeker, remarked at both internal contacts (ingress and egress) a thread of light darting out from each cusp and closing instantaneously about the planet, forming, for the instant, a slight protuberance on the Sun's limb. Mr. Landreth's time at second contact is the earliest of all the observations (12^{s} before the mean), and at third contact the latest (14^{s} after the mean); and with reference to this Mr. Boss asks, "Is it not possible that Mr. Landreth may have seen the planet projected against the solar 'sierra' or some part of the solar envelope not ordinarily visible in telescopic observations?" The phenomenon, however, is remarkably like that seen in transits of Venus and attributed to the planet's atmosphere. Both Mr. Landreth and his companion, Mr. Wilson (who used a 2·7-inch Dollond), saw, during the transit a small ill-defined light spot near the centre of Mercury's disk; it was slightly elongated or pear-shaped, and did not change its position on the planet in the course of 4½^{h}.

In connexion with the transit of Mercury, Prof. Holden has prepared an index-catalogue of books and memoirs on past transits, in which is given a brief statement of the physical phenomena recorded, in addition to the title of each paper.

OBSERVATIONS WITH THE WASHINGTON REFRACTOR*.—Prof. Asaph Hall has, in addition to his splendid series of measures of the satellites of Mars, made a number of valuable observations with the 26-inch refractor on the planets and satellites. He gives micrometer measures of Hyperion, Mimas, Oberon, Titania, Umbriel, and the companion of Sirius made in 1877 and 1878, and also notes on the disappearance of Saturn's rings, on Venus, and on the transit of Mercury. Nothing remarkable was noted with regard to Saturn's rings; and the observations, which include measures of the position-angle on 36 nights by Prof. Hall, and on 12 by Prof. Holden, show that Bessel's elements are very nearly correct.

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* Astron. Nachr., No. 2213.

Venus was examined from October 1877 to March 1878, and on several nights Prof. Hall noticed on the bright crescent part of the disk, and near its central part, an oblong portion that appeared brighter than the rest of the disk. This would appear to be the bright patch to which Mr. Brett first called attention, and which he explained as caused by specular reflexion from the surface, modified by large atmospheric diffusion (*cf.* 'Observatory,' No. 12, p. 360).

ENCKE'S COMET*.—This comet, which was unfavourably situated for observation in the northern hemisphere before perihelion passage, was picked up on August 3 by Mr. Tebbutt at Windsor, New South Wales, and by Dr. Gould at Cordoba, very close to the position given by Von Asten's ephemeris. Mr. Tebbutt describes it as a round nebula, about 2′ in diameter, with a gradual condensation towards the centre, and brighter than he expected to find it in his 4½-inch refractor, considering the Moon's presence, the low altitude, and the diffused twilight. Dr. Gould gives a similar description, stating that it was quite a conspicuous object, being on Aug. 10 comparable in brightness to a star of 8 mag. It was observed at Cordoba regularly till Sept. 6; but its light faded so rapidly that during the last ten days it was difficult to keep it in view while near the illuminated wires. On Aug. 17 its diameter was very nearly 1′. The comet passed its perihelion on July 11.

TEMPEL'S AND BRORSEN'S PERIODICAL COMETS †.—The second comet of Tempel 1873 *b*, which has lately passed its perihelion (1878, Aug. 27), was much fainter before the passage than had been calculated by the photometric formula; but its brightness after the passage was so much greater than the theoretical value that some good series of observations have been secured by M. Tempel, Dr. Schmidt, and others, and it is hoped that they may be continued to the end of November. The first periodical comet of Tempel 1867 *b*=1873 *a* may be expected in the spring of 1879; and M. Raoul Gautier, having discussed the observations in 1867 and 1873, has computed a set of elements, and is now engaged in calculating the perturbations by Jupiter since 1873, which, however, are inconsiderable, as the two bodies have been all the time at a great distance from each other.

Brorsen's periodical comet of 1868, re-observed in 1873, returns to perihelion on 1879, March 30, according to elements computed by Prof. Schulze. Some difficulty was experienced at first in satisfying the observations of 1873; and Prof. Schulze therefore recomputed all the perturbations, including those by Saturn, Mars, the

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* Astron. Nachr., Nos. 2222, 2229.

† Astron. Nachr., Nos. 2220, 2228.

Earth, and Venus. It appeared that the comet had been greatly disturbed by the last-named planet during the whole period of observation, 1873, Sept. 2 to Oct. 26, having approached Venus within one seventh of the radius of the Earth's orbit. The comet may perhaps be picked up at southern observatories in the latter part of February next. In April and May it will be much more favourably placed for observation in northern latitudes than it was in the year 1868.

The *Nautical Almanac* for 1882 (Murray, London, price 2*s.* 6*d.*) has just been published. We note two changes:—(1) the introduction of Le Verrier's tables of Uranus and Neptune, so that the places of all the planets as well as of the Sun are now deduced from Le Verrier's great work; (2) the places of *both* components of the double stars Castor, γ Leonis, α Crucis, and γ Virginis are now given in the list of Standard Stars. EDITOR.

Satellites of Saturn.

DISTANCES from Saturn's polar diam., in equatoreal semidiams.
— preceding or west, + following or east, N. north, S. south.

8^h G.M.T.	Encel.	Tethys.	Dione.	Rhea.	Titan.
Dec. 16....	+3·1 S	+4·9 N	−6·4 N	+8·3 S	−17·7 S
17....	−3·0 S	−4·7 S	+3·9 S	+4·6 N	−12·5 S
18....	−3·3 N	+4·3 N	+1·2 N	−6·7 N	− 5·2 S
19....	+3·6 N	−3·7 S	−5·5 S	−7·0 S	+ 2·9 S
20....	+1·4 S	+3·0 N	+6·1 S	+4·2 S	+10·5 S
21....	−3·9 S	−2·2 S	−2·5 N	+8·5 N	+16·7 S
22....	−0·4 N	+1·4 N	−2·7 S	−1·1 N	+20·4 S
23....	+4·0 S	−0·5 S	+6·1 N	−8·9 N	+21·2 N
24....	−0·6 S	−0·5 N	−5·4 N	−2·1 S	+19·1 N
25....	−3·9 N	+1·4 S	+1·0 S	+8·2 S	+14·4 N
26....	+1·6 N	−2·2 N	+4·1 N	+5·0 N	+ 7·4 N
27....	+3·5 S	+3·0 S	−6·4 S	−6·3 N	− 0·6 N
28....	−2·5 S	−3·7 N	+4·4 S	−7·3 S	− 8·5 N
29....	−2·8 N	+4·3 S	+0·6 N	+3·7 S	−15·0 N
30....	+3·2 N	−4·7 N	−5·1 S	+8·7 N	−19·1 N
31....	+2·0 S	+4·9 S	+6·3 S	−0·6 N	−20·0 S

Semiaxes of orbits expressed in semidiameters of Saturn's equator.

Dec. 31.	Mi.	En.	Te.	Di.	Rh.	Ti.	Iap.
Major	3·14	4·03	4·99	6·40	8·93	20·71	60·34
Minor	0·12	0·16	0·20	0·25	0·35	0·75	8·51

Iapetus is near its inferior conjunction, north of the planet, on Dec. 29. A. M.

Satellite of Neptune.

Major and minor semiaxes of apparent orbit on Dec. 31, 16″·7 and 5″·6. The satellite is near its greatest *n.f.* elongation in pos.-angle 40°·3, and near its greatest *s.p.* elongation in pos.-angle 310°·3, at the following times:—

Dec. 17	19^h	*s.p.*	Dec. 26	15^h	*n.f.*	Jan. 4	10^h	*s.p.*	Jan. 13	6^h	*n.f.*
20	17	*n.f.*	29	13	*s.p.*	7	9	*n.f.*	16	4	*s.p.*
23	16	*s.p.*	Jan. 1	12	*n.f.*	10	7	*s.p.*	19	3	*n.f.*

A. M.

Astronomical Memoranda, 1878, *Dec.* 15–1879, *Jan.* 14.

Sun. Dec. 15, sets $3^h\ 49^m$, rises $20^h\ 3^m$; Jan. 14, sets $4^h\ 16^m$, rises $20^h\ 2^m$.

Equation of time:—Sun *before* clock, Dec. 15, $4^m\ 38^s$; *after*, Dec. 25, $0^m\ 20^s$; Dec. 30, $2^m\ 48^s$; Jan. 4, $5^m\ 9^s$; Jan. 14, $9^m\ 16^s$.

Sidereal Time at Mean Noon:—Dec. 15, $17^h\ 36^m{\cdot}0$; Jan. 14, $19^h\ 34^m{\cdot}3$

Moon. Last Quarter, Dec. 16, 15^h; New, Dec. 23, $9\frac{1}{2}^h$; First Quarter, Dec. 31, 2^h; Full, Jan. 8, 0^h; Last Quarter, Jan. 14, 23^h.

Occultations:—Dec. 17, $15^h\ 5^m$, *q* Virginis R. 270°; Jan. 3, $3^h\ 48^m$, ε Arietis D. 11°, $4^h\ 25^m$, ε Arietis R. 301°; Jan. 6, $11^h\ 4^m$, 139 Tauri D. 35°; Jan. 7, $14^h\ 16^m$; ω Gemin. D. 111°; Jan. 9, $17^h\ 5^m$, o^2 Cancri R. 298°. The angles at disappearance (D.) and reappearance (R.) are reckoned from the *apparent* N. point towards the right hand round the Moon's circumference as seen in an inverting telescope.

Mercury is in inferior conjunction on Dec. 25, after which he is a morning star. He is at greatest elongation W. (24° 0′) on Jan. 16, when he rises in the S.E. at $18^h\ 24^m$, or $1^h\ 37^m$ before the Sun.

Venus is very near the Sun, having just passed superior conjunction.

Mars is a morning star in the S.E., moving *s. f.* through Scorpio.
Dec. 15, R.A. $15^h\ 26^m{\cdot}3$, Dec. 18° 30′ S., tr. $21^h\ 49^m$, rises $17^h\ 25^m$.
Jan. 14, R.A. 16 53 ·5, Dec. 22 38 S., tr. 21 18 , rises 17 20 .

Jupiter is an evening star, setting in S.W. at $7^h\ 13^m$ on Dec. 15, and at $6^h\ 1^m$ on Jan. 14. He is in conjunction with θ Capricorni on Jan. 7, 16^h (* 7′ N.). The Ecl. Reapp. of Sat. III. on Dec. 27, $5^h\ 5^m$, should be observed; there is also an Occ. Disapp. of Sat. I. on Dec. 20, $4^h\ 53^m$.

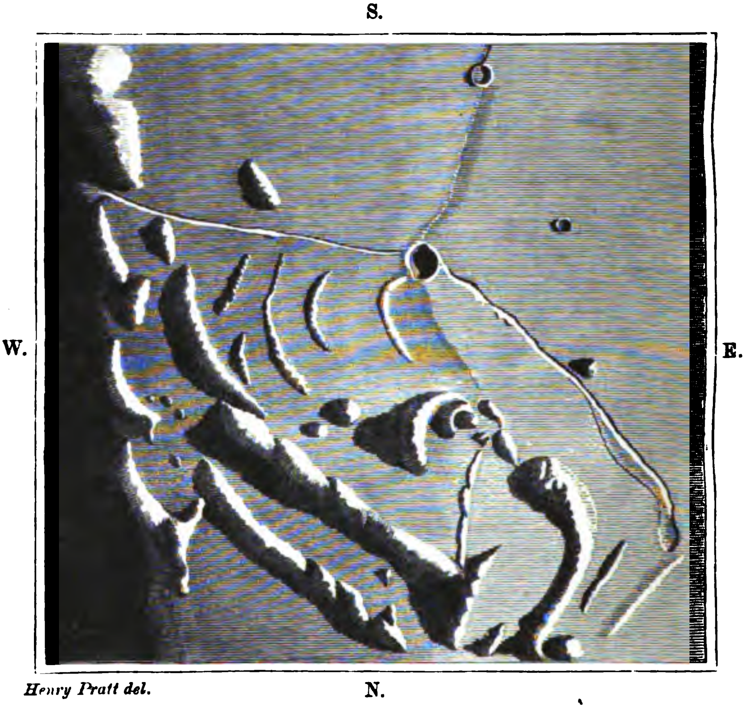
Saturn is an evening star, in quadrature on Dec. 18, and moving slowly *n. f.* about midway between α Pegasi and β Ceti,
Dec. 15, R.A. $23^h\ 50^m{\cdot}4$, Dec. 3° 37′ S., tr. $6^h\ 13^m$, sets $11^h\ 59^m$.
Jan. 14, R.A. 23 56 ·1, Dec. 2 53 S., tr. 4 21 , sets 10 10 .

	Outer Ring.		Inner Ring.		Ball.
	Maj. Axis.	Min. Axis.	Maj. Axis.	Min. Axis.	Diam.
Dec. 31 ……	38″·77	1″·53	25″·78	1″·02	15″·6
Jan. 20 ……	37″·58	1″·98	24″·99	1″·32	15″·0

The south side of the rings is visible, the elevation of the Earth above their plane being 2° 16′ S. and 2° 59′ S. on Dec. 31 and Jan. 20 respectively, and of the Sun 4° 54′ S. and 5° 13′ S.

Neptune is an evening star, about midway between α Ceti and β Arietis, 2° north of and slightly following ξ Arietis. EDITOR.

Publications received:—Mrs. Gill, *Six Months in Ascension* (Murray, London)—Capt. Abney, *Emulsion Processes in Photography* (Piper and Carter, London)—M. C. Lagrange, *De l'Origine et l'Etablissement des Mouvements Astronomiques* (Bruxelles, 1878)—J. N. Lockyer and Arthur Schuster, *Report on Total Solar Eclipse of April* 6, 1875 (Phil. Trans., part i. 1878)—Prof. H. Draper, *Total Solar Eclipse of July* 29, 1878 (Journ. Franklin Inst. 1878, Oct.). EDITOR.



THE REGION NORTH OF HYGINUS.

1878, OCTOBER 17th, 11^{h}.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 21. JANUARY 1. 1879.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1878, December 13.

Lord Lindsay, F.R.S. &c., *President*, in the Chair.

Secretaries: Mr. J. W. L. Glaisher, F.R.S., and
Mr. A. Cowper Ranyard.

The Minutes of the last Meeting were read and confirmed.

Mr. Glaisher reported that 62 presents had been received by the Society since the last Meeting, none of them, however, calling for any special notice, and thanks were voted to the donors.

Mr. Ranyard read an extract of a letter from Prof. Eastman to Mr. Schuster.

Mr. Christie read a paper by the *Astronomer Royal* "On the remarkable conjunction of the planets Mars and Saturn which will occur on 1879, June 30."

The relative positions of Mars referred to Saturn for every 15^{m} from 6^{h} to 9^{h} G.M.T. were exhibited on a diagram, which showed that the nearest approach was at $7^{h}\ 30^{m}$ G.M.T., when the planets were only 72″ apart. The nearest approach, however, could not be seen in England, as the planets would not rise till midnight, but it would be visible in Australia. A table had been prepared in which the local mean times corresponding to various phases of the conjunction were given for six stations, viz. Cape of Good Hope, Vienna, Madras, Sydney, San Francisco, Cordoba. Amongst the interesting observations that might be made, the Astronomer Royal suggested a comparison of the colour of the two planets. At the less favourable conjunction last year, he found that Saturn appeared of a sap-green; but possibly this was an effect of contrast due to the blaze of ruddy light from Mars. Mr. Marth had calculated the positions of Saturn's satellites about the time of conjunction, and had found that an observer in one of the Pacific Isles might see Iapetus very near Mars, though probably no occultation would take place.

Mr. Christie also read a note by the *Astronomer Royal* "On the determination of the Mass of Mars." The Astronomer Royal called attention to a paper by him in the 'Philosophical Transactions,' fifty years ago, in which, from a discussion of $10\frac{1}{2}$ years of Greenwich observations, he had found a value of the mass of Mars which agrees much more closely with Prof. Asaph Hall's recent determination than Laplace's value previously accepted, and he gave a statement of the various determinations made by different astronomers. As the paper appeared to have been forgotten, he recapitulated some of its results; in particular the diminution of the Moon's mass from $\frac{1}{60}$ to $\frac{1}{80}$, and the discovery of the famous long inequality of the Earth and Venus.

Mr. Neison. Would the effect, when observed in Australia, be to bring Mars nearer Saturn or to bring it further off? If it brings it nearer it will bring it perhaps $15''$ or $16''$ nearer.

Mr. Marth. The parallax would be comparatively trifling. A greater effect will be produced by the error in the 'Nautical Almanac' place of Saturn. Le Verrier's tables are used from the beginning of 1880, when the difference amounts to something like $20''$ in right ascension. Iapetus will be on June 30 near its greatest western elongation, where it appears pretty bright, and Mars will pass between it and Saturn. The correction needed is some $20''$, and that would bring Saturn further away from Mars, while it brings Iapetus so much nearer.

Mr. Dunkin. I think the tabular error is about $10''$ and not $20''$. During the last opposition it was $\frac{7}{10}$ of a second of time in R.A.

The error of Saturn changes very much during the season. About the time of opposition the errors are larger, but at quadrature they are much smaller. I think that Saturn at this time would be in quadrature and the tabular error would most likely be about $10''$, not $20''$.

Mr. Marth. On the 1st of January, 1880, $22''$ will be the error in heliocentric longitude, and Saturn is then near quadrature. I do not know whether the error increases or decreases. I have nothing to go by except the difference between the 'Nautical Almanac' place, as computed from Bouvard's and as computed from Le Verrier's tables, and I assume of course that Le Verrier is right*.

Mr. Gill. When I was at Ascension I had the good fortune to

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* [*Note by Mr. Marth.*] Le Verrier's correction of Saturn's heliocentric longitude, given in the 'Nautical Almanac' for the date Dec. 32, 1879, is $+22''\cdot2$, corr. of lat. $-11''\cdot5$, corr. of log. rad. vect. $-\cdot0000626$. If, in the absence of a direct calculation from the new tables, it may be permitted to assume that the same corrections apply also to the heliocentric place on June 30, the geocentric right ascension will have to be increased by $1^{s}\cdot84$, and the declination decreased by $0''\cdot8$. The rectangular coordinates of

observe, under circumstances which no European observer could ever enjoy, the conjunction of Mars and Saturn; and I observed it heliometrically, so that for the two days preceding and the two days following the day of conjunction I had accurate measures. In addition to that I had the advantage of seeing the planets, not only within a few minutes of each other, but actually side by side. The two planets were of the red and green colours referred to by the Astronomer Royal, and these were brought out in remarkable contrast. I had to diminish the brilliancy of Mars by using a wire screen placed over the segment of the object-glass, through which the light of Mars passed; otherwise it would have been too bright to make satisfactory measures.

The President. Do you think that the green colour of Saturn was due in any considerable degree to contrast with the yellow of Mars?

Mr. Gill. It was to some extent. When you shut out Mars you did not see the green colour so very marked.

Mr. Neison. There is a heliocentric error of Mars in longitude; and if I understand it rightly, it is only about 2¾″, whereas the parallax would be about 8″.

Mr. Gill. The parallax has nothing to do with the heliocentric error.

Mr. Neison. The effect of heliocentric error would shift Saturn a little one way or the other; whereas the effect of parallax, when the two planets were in the meridian, would be to bring Mars nearer Saturn.

Mr. Gill. It would be very small, because Mars at that time is far distant from the Earth.

Mr. Neison. It is a little further from the Earth than the Sun is. The parallax of the Sun is about 8″. It would be a horizontal parallax nearly if you were observing in Australia, with a latitude of

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Mars, Iapetus, and Titan, in reference to the centre of Saturn, are then the following:—

June 30.	*Mars.*		*Iapetus.*		*Titan.*	
h.	″	″	″	″	″	″
2 G.M.T. ……	529·6 W.	127·7 S.	501·6 W.	88·4 S.	67·0 E.	31·2 S.
4 " ……	355·2 W.	56·1 S.	501·7 W.	89·2 S.	72·3 E.	31·1 S.
6 " ……	180·7 W.	15·5 N.	501·8 W.	90·0 S.	77·5 E.	30·9 S.
8 " ……	6·2 W.	87·1 N.	501·9 W.	90·7 S.	82·6 E.	30·7 S.
10 " ……	168·3 E.	158·7 N.	501·9 W.	91·5 S.	87·8 E.	30·4 S.

Equat. Diam. of Saturn 17″·1, of Mars 8″·8.

The nearest geocentric distance between the centres of Mars and Saturn will accordingly be 83″·0, about $7^h 43^m$ G.M.T. If the 'Nautical Almanac' place of Saturn were correct, the nearest distance would be 71″·7, about $7^h 26^m$ G.M.T. The effect of parallax increases the nearest distance for Melbourne by 4″·0. It may be worth mentioning that the last close conjunction of the two planets occurred on April 18, 1817.

about 30° or 40°, and it just depends upon whether Mars would be in north or south latitude; and therefore I ask Mr. Christie whether the effect would be to bring Mars nearer Saturn or further off.

Mr. Christie. There was no attempt made to give any very minutely accurate representation. I think there would be some difficulty in representing the aspects at different stations; but I fancy Mr. Neison is under a misapprehension with regard to Mars and Saturn; the error of Mars is very small comparatively speaking, because the places in the 'Nautical Almanac' are taken from Le Verrier's tables; but the error of Saturn may be considerable, and in the case of a conjunction of Saturn and Mars you have to consider the errors of both planets.

Mr. Neison. If the top of the diagram is the north, the effect would be to increase the distance; and we should have a rather less interesting result than we might otherwise have had.

Mr. Christie. It would be so in Australia. The difficulty in applying parallax is that one wants to consider what the local time is; of course the local times are given in the numerical tables.

Mr. Dunkin. In Australia, where the conjunction will be visible better than anywhere else, at the time of least distance the planets will be at a fair height, and the parallax will not be more than 4″ or 5″, and this, therefore, can only affect within a few seconds the distance of the planets.

A vote of thanks was passed to the Astronomer Royal for his interesting communication.

Mr. Gill. Dr. Winnecke of Strasburg, in looking over the 'Historia Celestis Lucii Baretti,' found a very old observation of the Pleiades made in 1580 by Moestlin, with their right ascensions and declinations as obtained by that observer by very rough means. There are in all eleven stars observed by him, which Dr. Winnecke reduces to the epoch of Bessel's catalogue of the Pleiades and compares the results. The result shows that the places of those eleven stars were determined in 1580 with a probable error of only 2′ of arc. But the important point of the paper which he has communicated to the Society is that these observations were made *before the invention of the telescope*; and they confirm beyond all doubt a diagram which was prepared and exhibited some time ago in the 'Monthly Notices' by the Astronomer Royal repesenting the stars in the Pleiades as seen by his daughter. There can be no question that this astronomer, who never had a telescope, actually did see eleven stars, and those agree within very small limits of error with the places given by Bessel. Those who wish to read the paper more in detail will find it printed in the 'Monthly Notices;' and Prof. Winnecke has given a little woodcut which will also appear, no doubt, in the 'Notices.'

I have also a paper to read to the Society, a record of some observations which I made of the binary star α Centauri when I was in Ascension. I had intended to reserve them for publication with the rest of my work in connexion with the Ascension Expedition; but as many have applied for the results of those observations, I have thought it advisable to lay them before the Society without further delay.

There are two or three very curious points in the observation of the star *α* Centauri with the heliometer which may interest the Society if I briefly allude to them.

The first time that I looked at *α* Centauri was a few days after the erection of the heliometer in Ascension; and to my surprise, when I turned the telescope on the object, it had the appearance of an ill-defined single star (for the last time I had seen it was in connexion with Lord Lindsay's Expedition at Mauritius when the star was a good wide double one of 5″ or 6″ distance). That puzzled me very much. I did not think that the components had approached so closely together; and it was some little time before I thought what the cause could be; but by-and-by, on turning the position-handle of the instrument, the star suddenly developed into a double star. And the reason is this:—When we look at a single star with a telescope of 4-in. aperture, we get an optical disk of about 1″ in diameter; but if we look through such a telescope with one half of it cut off, or if the telescope is divided into two segments like the object-glass of the heliometer, we get a disk of elongate shape with oval rings round it, and with rays in the direction of greatest elongation, which are due to the portion of the glass which is necessarily cut away causing "interference." The major axis of the elliptical disk corresponds very nearly, though not quite, with the diameter of a disk due to a telescope of 2 in. aperture, and the minor axis corresponds very nearly to a disk due to a 4-in. telescope. Therefore what I really saw on looking into the heliometer, in the first place, was two elongated disks in contact at their greatest diameter; and then all the rings being mixed up together by the rather unsteady air (for it was soon after sunset and the heated clinker was giving off currents of hot air), I got an appearance which bothered me very much; but upon turning the axis of the heliometer round 90° and the air settling down into a somewhat more steady state, I got the two oval disks neatly separated with a fair space between their least diameters. The object-glass was now in its best condition for observing the components; and I got two disks fairly distinct and separate, and capable of accurate measurements, as the results of the measures will show.

In the ordinary methods of observing a double star with the heliometer, you endeavour to place the four stars in a line and at equal distances; but if you apply this to a very bright and close star like *α* Centauri, you get a very unsatisfactory thing to measure, because it is quite impossible to adjust the two halves of the object-glass so that you shall have the optical centres of those two halves in exact coincidence. You may shift them up and down by the screws for the purpose; and if you do succeed in getting the telescope so that the two halves shall produce a single image, that adjustment is soon changed with a change of temperature. During the Ascension observations there was generally an error of 2″ of arc in the adjustment of the single image. Consequently you could hardly arrange the four stars in a line. What I fell back upon was this: the images being accompanied with a long diffraction-ray, I endeavoured to move one pair of stars relative to the other pair until the four diffraction-rays gave parallel lines of equal distance from each other; and in that way I was able to get very exact measures indeed of distance; but when the time came that I was obliged to observe α Centauri in daylight, those rays were no longer visible; so that I was driven to the common expedient of placing the first star of one pair under the second star of the other, and then reversing the operation; and thus I got very good measures of distance indeed. A table which I have prepared shows the result of those measures; and in due time the details will be published to show that they are quite honest good measures fairly made. The first distance that I got was 2″·187, then 2″·125, 1″·935, 1″·950; later on 1″·863, 1″·773, steadily diminishing all the way. The last measure gave 1″·423; but the definition was extremely bad. It was made about midday when the sun was creating a great many vertical currents; but there is a very evident change of half a second of arc.

Then as to the position-angle, I have given a description of the method of measuring. There was a change of 20° of position-angle between 1877·56 and the last observations at the end of the year. So that it is very evident that those observations were made during an exceedingly interesting period of the orbit; and it is interesting in connexion with that to read Mr. Ellery's letter in 'The Observatory' for August 1878, in which he mentions that from measures made on the 8th of May, the star was again increasing its distance. There were nine or ten measures altogether; and I hope they will be a valuable contribution to our knowledge of this interesting object.

Mr. Thornthwaite. I would suggest that if you had used a cylindrical lens you could have got a brighter star with a longer line, which is a very handy position for making an observation. I think it might be a suggestion to try on the heliometer when you are examining stars again to use a cylindrical lens just in front of the eye-lens.

The President. With regard to the difficulty which Mr. Gill has mentioned with close stars, I wish to draw attention to the diffraction-line which he drew perpendicular to the division of the object-glass. I think he will remember that on the evening when we were at work, one of us thought he had discovered a comet. That produced great excitement for some little time, until we traced it back to α Lyræ at a distance of some 15° or 20°. The light of that star had fallen within the division between the two segments of the object-glass, and was reflected, and produced what seemed a most beautiful comet.

Mr. Christie read a note on a phenomenon seen in the occultation of a star at the Moon's bright limb. While observing the disappearance of one of the Pleiades, a 4 mag. star, on Nov. 10, he was surprised to see that when the star apparently came up to the Moon's bright limb, the limb appeared to recede from the star for the space of 3^s or 4^s, and the latter finally disappeared in a sort of luminous haze through which it was seen with greater and greater difficulty as it advanced. At the disappearance the star appeared to be bisected by the limb, the rings and rays and half the disk being cut off on the side next the Moon. These phenomena he explained as naturally resulting from the theory propounded by The Astronomer Royal to account for the apparent projection of stars on the Moon's disk. In observing the occultation of a 5-mag. star he had found a graduated dark wedge useful to diminish the glare of the Moon's light.

Mr. Christie. I also wish to mention that a large Half-prism spectroscope, which Mr. Hilger has just made for Mr. Ellery, has been brought here this evening for the inspection of the Fellows. Mr. Hilger has worked with great zeal at the improvement of plane surfaces, and has succeeded in making prisms for this spectroscope which seem to be absolutely perfect in this respect. In this instrument either one, two, or three half-prisms can be used at pleasure, giving a dispersion of four, sixteen, or sixty ordinary prisms of 60°.

Mr. Brett. The explanation of Mr. Christie of the encroachment of the star on the disk is extremely interesting; but I beg leave to ask him why it does not always occur when a large star is occulted?

Mr. Ranyard. Several observers have noticed that when the Satellites of Jupiter come up to the limb of the planet and are occulted, the part of the limb next the Satellites appears to be flattened. Owing to the greater curvature of the limb of Jupiter than of the limb of the Moon, it is easy to understand how diminution of the irradiation-edges would give the appearance of flattening. Admiral Smyth and Mr. Webb have both, I believe, observed this flattening. A similar explanation to that suggested by Mr. Christie will no doubt also account for an observation of Mr. Todd in Australia, who some time ago described one of the Satellites as being seen through the limb of Jupiter; and some very remarkable physical deductions were drawn from the observation with regard to the atmosphere of the planet.

Mr. Gill. I think I understood Mr. Christie to say that the star was approaching a hollow in the Moon. It may have some bearing on the possibility of the existence of a lunar atmosphere at a certain depth.

Capt. Noble. I see announced for the first time that Mr. Todd, whose observations fill a great many pages, observed with a ricketty equatoreal and a chronograph that will not act; therefore I think that any phenomena to which he invites attention may reasonably be put down to some other cause than that of actual physics.

The President. Is there any evidence that he does work with a ricketty equatoreal?

Capt. Noble. Only I see his statement that his chronograph was not very good, and the instrument required a better R.A. clamp; but he says, "with a better R.A. clamp, which I will make, and a new chronograph, which I have made since, I shall have better observations in future." At all events his observations must, *pro tempore*, be received *cum grano salis*.

The President. I think, even with a ricketty instrument, some very good observations may be made, and I know have been made, by Dr. Tempel at Florence, who is now making use of an equatoreal 14 feet long, and if you touch it with your little finger it will vibrate for ten minutes.

Mr. Gill. I think Capt. Noble's criticism on Mr. Todd's observations is a very unfair one. Mr. Todd is one of our most careful observers, and he has an excellent telescope. Mr. Todd's remarks apply to his clamp in Right Ascension, and I must remind Capt. Noble that a telescope is not generally clamped in Right Ascension when used in observing the phenomena of Jupiter's satellites. (Laughter.)

Mr. Neison. In confirmation of Mr. Christie's remarks, I have myself seen on more than one occasion a sort of gradation in the light from the Moon's limb. I have noticed the star come up to the limb and disappear within the apparent optical limb of the Moon. If you were to take a transit you would take the outer or false limb, whereas if you observe with a very high power and under good conditions, you will see, I think, that there is a true limb within, with all its irregularities sharply marked, and you will see outside a sort of fainter copy of it forming a false limb. You notice, as a rule, that if the star is more than about the fourth magnitude it goes right within this false limb and then disappears. If you come to reduce the observations you will find a very appreciable difference between bright stars and faint stars. You will find that faint stars disappear rather more than half a second of arc further away from the centre of the Moon than bright stars. Thus a bright star will give you one value for the distance of the star from the centre of the Moon when it disappears, and a faint star will give you a value half a second greater or even more. I concur with what Mr. Ranyard has said about Jupiter. Mr. Todd, I believe, really did see what he said, that is to say, the satellites appeared gradually to approach within the limb of Jupiter and disappeared a little way within, as if they were seen through a sort of fringe round Jupiter. That, however, is no more than has been seen by other observers besides Mr. Todd, for instance by Mr. Wray and others; and I think it was Mr. Dawes who also saw the same phenomenon about ten years before Mr. Todd made his observation.

Mr. Christie. I will endeavour to answer the questions which have been put. First, Mr. Brett asked me why those phenomena are not always seen. That may be rather a difficult question to answer. When I observed the occultation of a bright star at the bright limb myself, I distinctly did see the projection of the star on the disk. I believe that the phenomenon of projection has been frequently referred to the colour of the star, that is to say, the star is seen distinct from the Moon's limb by virtue of its difference of colour. In the case I refer to, when I was expecting nothing of the sort, I saw the star Aldebaran go on and advance over the Moon's disk, and there was a distinct difference in colour from the Moon. At the same time a great deal may depend upon the state of the observer's eye, and that is the explanation which the Astronomer Royal has offered. The observer may either be conscious of this false light at the Moon's limb or not, according as he directs his attention on the Moon or on the stars.

Mr. Gill asked me whether the star disappeared in the notch. I thought it only fair to mention that there was a notch at the limb, but the chief point about that was that the definition was good enough to enable me to see this notch. I do not think the phenomenon can be accounted for by any atmosphere, because the notch was very small compared with the extent of the sort of atmosphere round the Moon.

Mr. Neison remarked that there was a difference in the diameter that came out, when the observations were reduced, between a bright star and a faint star. I may mention that Mr. Dunkin has reduced these observations. I observed one star of the fourth magnitude and another of the fifth magnitude; the observations of disappearance at the bright limb and reappearance at the dark appear to agree very well.

The President. I think, on the question of the difference due to the magnitude of the stars occulted, I can quite bear Mr. Neison out, that if it is assumed that the small star actually does disappear sooner than it should do theoretically, of course what Mr. Neison says will be true; but I cannot help thinking that the eye is merely overpowered by the brightness of the limb. I have myself found a difficulty in seeing a small star as it gets right up to the limb and disappears. I have never yet been fortunate enough to see a real projection on to the Moon's limb, although I have watched many occultations in the hope of seeing such projection. At my observatory we computed the time for the occultations and sat up for them; but the night was most completely and absolutely dark, otherwise I might have had the interesting observations which Mr. Christie had.

Mr. Neison. I think Lord Lindsay's explanation is probably the true one, that the faint star disappears through the eye being overpowered with the brightness of the Moon's limb.

The President. At to what fell from Mr. Ranyard with regard to the apparent flattening of the disk of Jupiter when the satellites came up to it, I think you can see that mechanically also. If you have a white disk projecting on a black ground, and you have a very small disk of some colour brought up to make the contact, such as we make in the transit of Venus, the greater one will be apparently flattened at the same time that the smaller disk is brought up against it.

Mr. Downing read a papar on the Reduction of the North Polar Distances of the Cape Catalogue for 1860 to Auwers' Standard. When comparing catalogues together for such purposes as the determination of proper motions, it is of course necessary, where extreme accuracy is desired, to reduce the places of the stars given in the different catalogues to a uniform standard, so that the systematic discordances peculiar to each individual catalogue may be thus eliminated. The standard usually adopted for North-Polar Distances is that given by Dr. Auwers in No. 1536 of the 'Astronomische Nachrichten' (also published in the 'Connaissance des Temps,' 1868); and in this paper the corrections are given which it is necessary to apply to the N.P.D.s of the Cape catalogue for 1860 to reduce them to this standard.

The Cape catalogue has first been compared with the Greenwich Seven-year catalogue for 1860. As Mr. Stone has reduced all the stars in the Cape catalogue which are common to it and the Greenwich catalogue with the proper motions given in the latter, no correction is necessary on this account, and the places in the two catalogues have been used without any alteration. There are 385 stars common to the catalogues which are available, after rejecting all those whose places in either catalogue depend on a single observation. These 385 stars have been arranged in order of N.P.D. and then taken in groups embracing about 5° each, and the means of the N.P.D.s and of the differences between the catalogues taken for each group. The differences have then been plotted down and a curve drawn through the points, which may be taken as representing the systematic differences between the catalogues. The differences extending from N.P.D. 48° to 124° are given in a table for every 4°, and by applying the corrections to the Greenwich catalogue given by Dr. Auwers, we have the corrections applicable to the N.P.D.s of the Cape catalogue to reduce them to the standard. For the southern stars situated between N.P.D. 120° and the South Pole, the Cape catalogue has been compared with Henderson's catalogue published in the tenth volume of the 'Memoirs,' as Dr. Auwers uses the latter as a provisional standard, there not being sufficient materials for the formation of an independent standard for these southern stars. There are 67 stars available for the comparison with Henderson; and as it might be considered that this number was not sufficient for a trustworthy comparison, the catalogues have also been compared indirectly by means of Johnson's St.-Helena catalogue, there being 202 stars common to the Cape catalogue and Johnson, and Dr. Auwers having given the reduction of Johnson to Henderson derived from 149 stars. No correction has been applied to Henderson's places for the effect of the proper motion corresponding to the fraction of the year; but for Johnson's places two years' proper motion has been applied to every star, as the places are given for 1830·0, and 1832·0 may be taken, in the mean, as the mean date of observation. The mean of the direct and indirect comparison between Henderson and the Cape catalogue has then been taken as the adopted reduction to Henderson.

Mr. Green read portions of a letter he had received from Professor Trouvelot, Cambridge, U.S., in which reference was made to the singular agreement between the drawings of Mars made at Cambridge and Madeira. The letter also contained a reference to the dark canals in the drawings of Professor Schiaparelli, of Milan, tracings of which had been sent to Professor Trouvelot. Professor Trouvelot's observations gave very little support to Signor Schiaparelli's representations of these canals. According to Signor Schiaparelli, these canals became visible after opposition; but Prof. Trouvelot has observed the planet on every clear night "from April 1877 till May 1878, and has made during this period 218 observations, and obtained 212 drawings, without being able to see them."

Mr. Green continued his remarks by quoting from the 'Transactions' of the Royal Dublin Society a statement by Dr. Dreyer, of the Earl of Rosse's Observatory, Parsonstown. Referring to the surroundings of the Lockyer sea, Dr. Dreyer says, "It appeared to be joined to the De La Rue sea by an extremely faint and diffused band, but this was considered rather doubtful." Mr. Green said he had observed exactly this appearance of a uniting band during the earlier part of the evening of Sept. 1, 1877, but that as the definition of the night improved, this supposed band resolved itself into a definite dark spot, lying between the Lockyer sea and De La Rue sea, and clearly separated from both; and he argued from this that it was possible the dark canals seen by Professor Schiaparelli may have had a somewhat similar origin, and that many discrepancies between the various drawings of the planet may be due to the deceptive appearances produced by atmospheric disturbance.

Mr. Knobel. I have seen this map, and would suggest, before it is printed, it should be referred to a Committee of one or two, who are fully acquainted with the drawings that are extant of Mars, that they may discuss them impartially, and see what they represent, and make what alterations or corrections may seem necessary.

Mr. Ranyard. It is a very serious thing to publish the chart of a planet because it has a tendency, no doubt involuntary, to affect subsequent drawings—a tendency which perhaps observers are not able themselves always properly to estimate. In this drawing what has been called the great Equatoreal Girdle of the continents of Mars appears not only to be a little more solid, but a little more towards the north. It seems to throw the mass of the continents of Mars more into the northern hemisphere than Mr. Proctor's map of Mars. In his map the chief mass of the continents is also in the northern hemisphere; and there, as here, is a ring of sea round the pole, which again has a ring, or nearly a ring, of land around it, and again a ring of sea, and then the Equatoreal Girdle of continents, and that is the same at either pole. The size of the two polar ice-caps differs, but probably that may be owing to the very remarkable difference between the cold at the two poles. The north pole of Mars, I think, is being turned towards the Sun when Mars is in aphelion; and as there is a greater ellipticity in the orbit of Mars than in our orbit, there is a much larger northern than southern development of ice. There is also to be considered the time at which Mr. Green made his drawing; but what I want to draw attention to is that the appearance of snow-action—and it may be of recent snow-action—seems to be greater at the North Pole of Mars than at the South Pole. In this map Mars is a little more like our Earth than appears in the Mercator's projection, published by Mr. Proctor some years ago, and which was made from drawings by Mr. Dawes.

The President. By turning the points into lines, Mr. Green suggested that bad definition would produce a new description of astigmatism, because practically it would come to that. Although the ordinary effect of astigmatism is confined to a certain angle in the eye, bad definition, according to Mr. Green's theory, would produce astigmatism at any angle in which it was seen. I understood Mr. Green to say that certain points became a line.

Mr. Green. Yes; and the answer I would desire to make to that would simply be that they are connected more or less with atmospheric vibrations; that is to say, if you have a series of points in a line, the mere circumstance of the atmospheric vibration between the one and the other would very likely connect them and produce the idea of a line. I find in a great many portions of Mars, especially since I went carefully over the chart-formation, comparing it with M. Schiaparelli's drawings, there were faint hints of connexion here and there, sometimes stronger and sometimes weaker, but quite sufficient, with imperfect sight or imperfect definition, to produce the idea of a continuous line; and therefore it was quite possible that when an observer, perhaps not very well accustomed to draw exactly what was seen, saw a something, that something was turned into a line, whereas what was really seen was very far from a sharp line.

The President. So far as I recollect M. Schiaparelli's drawings, those lines or connexions were not only vertical, they were at every angle.

Mr. Green. Yes.

The President. So that any disturbance taking place would probably be in one direction, either vertical or horizontal, and not a disturbance vertical and horizontal and at an angle at the same time.

Mr. Green. It would depend upon the direction in which the vibrations took place as to whether the connexion between the points would be most distinctly seen. With regard to Mr. Knobel's proposition, I shall be quite delighted if anybody will kindly help me.

Mr. Sadler. May I suggest that with the telescope that M. Schiaparelli used it would be almost impossible to see the minute canals that he has in his drawings of Mars. I have looked at that map of Mars, and those canals appear to be under $\frac{1}{10}$th of a second in diameter, and with an 8-inch aperture it is perfectly impossible to see canals of that size.

Mr. Neison was called on to read a paper on a "General Solution of the Problem of Disturbed Elliptic Motion." The paper gives an account of a method for completely determining the perturbations of a body moving in an elliptic orbit and disturbed in its motion by the attraction of one or more distant bodies. Suppose the body to be a planet moving round the sun, or a satellite moving round a planet, if there were no other bodies it would move in a uniform ellipse. But suppose that there are other planets or other satellites in the system, the given planet or satellite will no longer move round its primary in a fixed elliptic orbit, but will be disturbed in its motion by the effects of the attractions of the other bodies. Its motion may be represented by supposing it to move steadily in an ellipse—not in a fixed ellipse, but in an ellipse which is constantly changing in form and position. The problem is reduced, therefore, to completely determining these changes in the form and position of the ellipse.

It has not as yet been found possible to determine these perturbations or changes in the form or position of the ellipse in a finite form, so that we are obliged to determine their values by successive approximations. We first determine the effect of the attraction of the disturbing bodies on the planet as it moves in its undisturbed orbit, and so compute what are called the perturbations of the order of the first power of the disturbing forces, because each of these perturbations depends on the first power of the mass of the disturbing body. Secondly, we determine the effect of the attraction of the disturbing bodies on these perturbations of the moving planet. These are called the perturbations of the order of the second power of the disturbing forces, for they involve the second power of the mass of the disturbing body. In this manner, by successive approximations, it is theoretically possible to completely determine the perturbation to any order of accuracy.

For the planetary theories this method can be employed in practice, for it is not necessary to go beyond the second approximation; but for the moon it has never been considered practicable, for it is necessary to consider the fourth approximation, and the method becomes so very complicated under these conditions as to be unmanageable.

By suitably transforming the usual elements of the elliptic orbit into another system of elements, canonical in form and possessing the requisite properties, Mr. Neison has been able to obtain a general expression for the perturbations complete to any order of accuracy. By giving particular values to the constants in this expression the value of the perturbations can be at once written down to any order of accuracy, and so in practice a complete solution be obtained.

Mr. Thornthwaite read a paper by Mr. Levander describing an instrument with a variable diaphragm for use in solar and sidereal observations. He said :—"The idea of this frame is to get as perfect as possible a size of aperture for various purposes, such as observing the Sun, shutting out a brighter star when you have to observe a faint companion. The principle is two squares, which can be very accurately made, and by moving one over the other you get the contraction of the telescope always in the centre of the field and of any given size. I have not found the effect of a square quite so bad as a triangle. I believe Mr. Levander intends to make use of it in various ways, putting it, for instance, in the focus of an eyepiece and diminishing the field and also the light of the Sun.

"In respect to my remarks about cylindrical lenses, I should wish to add a few words of explanation. If you look through a cylindrical lens at a pair of stars, instead of getting each star as a disk you get it as a line, and obviously of less diameter with greater length. I think it is very plain that it is possible to measure with greater accuracy a pair of lines than a pair of disks in a double-image micrometer. You can bring the one line very truly over the other, and also when you revolve the cylindrical lens 90° you get the stars in one line and can measure very faithfully the position-angle. I would suggest to Mr. Gill that a modification of this plan might be tried with the Heliometer."

Mr. Gill. I am obliged to Mr. Thornthwaite for his explanation. I have used the cylindrical lens in a great many ways before, but I think he forgets that the problem is measuring angles and distances in the heliometer, and if he were to try it with the heliometer he would find a great many difficulties. If he likes to talk with me afterwards he will find that it is impossible to apply a cylindrical lens with the heliometer. He refers to the advantage of superposing one line upon another. The principle of superposing one disk upon another is the worst possible method of measuring. The great thing in measuring distances with the heliometer, where you have large distances to measure, is to get the coincidence of two disks by causing one disk to pass rapidly over the other, and not by setting one on the top of the other, which would introduce a cause increasing instead of diminishing the error.

Mr. Ranyard. I should like to correct a mistake I made at the last meeting, owing to an oversight. Mr. Schuster read a paper on the recent Eclipse, in which he drew attention to a matter which has been observed by more than one polariscopic observer during recent eclipses, viz. that the polarization of the corona does not decrease regularly as you pass outwards from the Sun's limb, but that there is an area of maximum polarization at a certain height above the Sun's limb. Mr. Schuster showed that the polarization of the light dispersed by small particles in the corona should increase as you pass away from the Sun. His proof only applies to the light dispersed by single elements of the corona; but the same thing is true, as I have shown in the Eclipse volume, if we take into account the polarization of the light derived from all the elements of the corona seen in the same line of sight by an observer at the Earth. Mr. Schuster's method of reconciling the observed facts with theory is to suppose that in the higher parts of the corona a condensation of matter takes place, and that the light is dispersed by the larger particles according to a different law. It seems probable that any such change in the physical condition of the matter of the corona would be accompanied by a change in the intensity of the light dispersed; but there is no evidence afforded by the photographs of any sudden change of brightness corresponding to the area of maximum polarization. In fact there is nothing which looks like concentric structure indicating regions of dissociation and condensation such as theorists have frequently referred to. The more probable explanation of the decrease in the intensity of the polarization in passing outwards is that the polarization of the faint light of the outer corona is drowned by the light dispersed by our atmosphere through which the corona is observed. This is the explanation I have suggested in the Eclipse volume; and when we consider the circumstances under which the corona is observed, I think it seems the most probable explanation.

During totality the atmosphere between the observer and the Sun is lit up by light derived from clouds upon the horizon. And the sky about the Sun's place disperses a considerable amount of light, which is found to be faintly polarized in a plane perpendicular to the horizon. In considering the polarization of the light derived from any part of the visible corona, we shall have to consider what will be the polarization of the light which is compounded of the light derived from that part of the corona and the light dispersed by the atmosphere. Now though the polarization of the light of the corona increases rapidly as we pass away from the Sun's limb, the intensity of the light of the corona decreases very rapidly; and the consequence is that the radial polarization of the total light diminishes outside a certain point. During the recent eclipse, and also during the eclipse of 1871, it would seem that the radial polarization was very clearly visible close to the Sun's limb, and that the intensity of the radial polarization did not decrease very rapidly in passing from the area of maximum polarization down to the Sun's limb so as to cause the area of maximum polarization to appear as a marked feature. In fact some of the observers during the recent eclipse described the polarization as decreasing regularly from the limb outwards. Now this shows that there cannot be any considerable mixture of unpolarized light with the light dispersed by the small particles of the lower corona, for such an admixture would tend to obliterate the polarization of the lower parts of the corona so that there would be a rapid increase of polarization in passing from the limb outwards. This reasoning becomes more striking when we consider all the other causes which must tend to obliterate the polarization near the limb, such as the large angle which the photosphere subtends to particles floating in the lower corona, and the admixture of incandescent particles with the coronal matter. We may therefore assume that the light of incandescent gas which would be unpolarized does not form a large proportion of the light of the lower corona, and consequently that the monochromatic images of the corona must be very faint compared with the total light of the corona.

Mr. Brett. With regard to the recent Eclipse, we were peculiarly fortunate in that our Secretary was able to observe it under very favourable circumstances. It has hitherto been the practice of Fellows of this Society to relate to us their experiences on such an occasion, or else to present them to the Royal Society. But the only information that we have got about this Eclipse has been from the American newspapers, in which very interesting observations are attributed to Mr. Ranyard; and I am sure it would be very gratifying to the Society if he would explain to us how it was particularly that he made his photograph of Young's stratum. Most of us are not aware that Mr. Young has got any stratum. He is a distinguished Associate of this Society, but that has no bearing upon his stratum; and I should like to know what it is, and to have Mr. Ranyard's description of his photograph. Another point interested me very much. Those observations are said in the American papers to have been made with a 13-inch camera and a 6-ft. focus: I have no idea what such a camera can be. I did not know that a camera had any focus; and perhaps the President will allow me to ask whether he thinks that it is treating us fairly, or with Mr. Ranyard's usual courtesy.

The President. Those observations require a great amount of thinking and reduction before they can be brought forward. The observations which were taken of the Transit of Venus were taken four years ago by the Government, but we cannot say that they are really published yet.

Mr. Brett. If they had not been communicated to the American paper I should not have said any thing about it.

The President. The Americans are wonderful people for getting information.

Mr. Christie. With respect to the Transit of Venus observations, the Astronomer Royal was careful to inform the Society what had been done. Without discussing the observations he carefully stated what results had been obtained or had not been obtained, without committing himself any further.

The President. I will withdraw the Transit of Venus, which was the first thing that occurred to me. I would refer to my own observations in 1870 at Cadiz.

Capt. Noble. Mr. Brett must be troubled with an exceedingly short memory; but if he would care to exercise it he might remember that a friend of his own, a Fellow of this Society, went out to observe the Eclipse of 1871. The first information that we had came from Col. Tennant, who came into this room within twenty-four hours of his landing in England and gave us an account of it.

But that friend of his was hawking about his observations to any body that would pay to hear his lecture upon them, though we never heard a word about them. As a personal attack has been made upon Mr. Ranyard, I think it right to say this.

Mr. Ranyard. I do not think that a personal attack was intended. The photographs are now on the table; and if Mr. Brett cares to look at them afterwards, he can do so. They were taken with a lens 13 inches in diameter, in the focus, 6 feet away from the lens. The camera was mounted equatoreally in the old English fashion; the box in which the camera went out served for the polar axis. The reason of my not having read a paper on the Eclipse before is that some years ago I undertook to edit the observations of the Eclipse of 1870. Shortly afterwards the observations of the Eclipse of 1860, many hundreds of which had been collected by the Astronomer Royal, were put into my hands to edit also. After that came the Eclipse of 1871; and at the same time it was considered that it would greatly improve the whole if a collection of the physical obserations of eclipses generallywas made. This work has taken up my time for nearly seven years. I thought it was right, with the approval of the Astronomer Royal, to go to America and make use of the beautiful camera lent me by Mr. Esdaile, and to leave the volume for the time that the journey would take. Since my return I have been giving the whole of the time that I can give to such matters to the Eclipse volume, in order that it may be in the hands of the Fellows as soon as possible; and although they are naturally becoming a little impatient, I think that they would wish me to finish what I have undertaken.

The President read a note on some hitherto unnoticed features near the lunar crater Hyginus, observed with a refractor of 15½ inches aperture. On Oct. 16 the terminator passed over Hyginus and the configurations were well shown. Six drawings were made. In sketch No. 1 there is no trace of the crater in the S.W.; but in No. 3 a curved ridge is shown, and in No. 5 part of the wall of a shallow but regular crater.

On the motion of Mr. Talmage, seconded by Mr. Brett, Mr. Common, Mr. Esdaile, and Dr. Wentworth Erck were re-elected Auditors.

The following papers were announced:—

J. E. Gore. On a new Variable Star, ι Andromedæ.

W. H. Pickering. Total Eclipse of the Sun, July 29th, 1878.

Prof. C. A. Young. Measures of the Diameter of Mercury made at the Princeton Observatory, May 6th, 1878.

Rev. S. J. Perry. Phenomena of Jupiter's Satellites, 1877–78, observed at the Stonyhurst Observatory.

The following were ballotted for and duly elected Fellows:—

The Rev. Charles Heaven and Mr. James Whewall.

On Dr. Klein's supposed new Crater, and the region north of Hyginus.

WHILE persistent cloud and haze continue generally to obscure the Moon, confusion to some extent seems to hang over the observations that have been made of the object called Klein's crater. And as this confusion appears to puzzle such eminent selenographers as Mr. Birt and Mr. Neison, it is not wonderful if mere observers, like the writer, find great difficulty in reconciling the observations with each other, or Dr. Klein's statements with the Moon itself. The existence of this confusion will be apparent to anyone who will patiently compare all the published observations. Indeed it seems doubtful if some observers' notes do really refer to the object they are intended to describe.

This undesirable state of the matter is very greatly chargeable to the misleading engravings professing to indicate the position of the object to which we have been treated in this country—a fact to be regretted because, in combination with the continually unfavourable weather, it has helped to place English observers at a disadvantage. The dates of Dr. Klein's observations also have been wrongly stated by more than one scientific journal; so that it is very desirable that observers should take advantage of all favourable opportunities in order to prevent the matter from lapsing into the difficulties which beset the elucidation of the famous case of Linné.

The actual existence of an object in the locality named by Dr. Klein is proved not only by his own observations, but also in this country by those of Messrs. Neison, Knott, and Sadler, and supported by existing photograms. Mr. J. Rand Capron, F.R.A.S., of Guildford, has also obtained a really fine view of it. The following are his notes relating to it, with which he has kindly favoured me:—

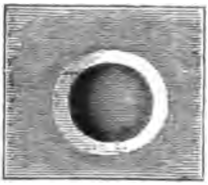
"1878, Sept. 6th, Moon's age $9^d\ 18^h$, midnight. Hyginus, and adjoining ridges, and some small craters well seen and sketched. Klein's crater was not observed."

"1878, Sept. 16th, 9^h G.M.T. Moon's age $19^d\ 15^h$. Neighbourhood of Hyginus examined and sketched. (Mr. Pratt was with me.) Klein's crater was unseen."

"1878, Nov. 2nd, $6^h\ 45^m$ G.M.T. Moon's age $7^d\ 19^h$. The district was not quite on the terminator. The field was steely grey in tint, only moderately lighted, and the ridges and craters stood out strong and sharp. Definition good throughout, though the air was not steady. Bore eyepiece E well. An object was found in the position assigned to Klein's crater. A moderately sized, very slightly oval, cup-like cavity, of greyish-black tone, with an indistinct lighter margin or rim. The cup was more deeply shaded on the side towards the Sun, while the rim or margin was lighted up on the opposite side. No focussing would give detail; and this nebulous appearance was in great contrast with the surrounding objects."

"1878, Nov. 10th, 9^h 30^m G.M.T. Moon one day after full. No details could be got out of the district in general, but there was much haze."

"The instrument used was a 6-inch equatoreal by Cooke. Eye-pieces Browning's achromatic B and E; powers 178 and 378."



J. Rand Capron del.

KLEIN'S CRATER. 1878, Nov. 2nd, 6^h 45^m.

Mr. Capron further remarks:—

"The impression conveyed to my mind was that the object could be most easily overlooked, except under very favourable conditions of lighting and air. I have a lunar map by Russell, the crayon painter of Guildford; it is quite fifty years old. Upon it Hyginus and its neighbourhood are fairly shown; and near where Klein's crater should be there is an indistinct dark spot, but I am not at all certain it is more than an accidental darkening in the shading on the stone."

The difficulty of observing Klein's crater might so far be accounted for if the observations indicated any thing like the occasional local obscurations which I discovered during my seven or eight years systematic scrutiny of the floor of Plato. But they do not. At least I will endeavour to show that other reasons are quite sufficient to explain it.

The fact that *its visibility is sensitive to slight variations in the angle of illumination* is proved, at least to my own satisfaction, both by Dr. Klein's observations and those of Mr. Capron.

The following Tables will make the matter clear:—

Date.	Alt. of Sun.	Klein's remarks.
1878, April 9.	3° rising.	Extremely distinct. Obvious as Hyginus. Deep black.
1877, May 19.	2 „	Large black crater without wall.
1878, April 10.	13 „	Grey diffused spot.
1877, Nov. 13.	17 „	Undefined border. Black nucleus.
1877, Dec. 13.	19 „	Hardly distinguishable. Dull spot.
1877, July 18.	22 „	Large black crater. Vaporous border with nucleus.
1877, Nov. 14.	27 „	Not a trace. Air remarkably clear.
1877, July 19.	28 „	Disappeared. Perhaps an undefined grey spot.
1878, March 15.	56 „	Not a trace. Other objects distinct.

Here it will be seen that the remarks vary very closely with the altitude of Sun above Klein's crater and that when the altitude is below 10° rising the object is well seen, that when it is between 10° and 22° the object can still be seen but undefined, and that generally above 22° not a trace of the object is visible.

The observation of 1877, July 18, is the one among nine that disagrees very slightly with the general rule; but may not that be well accounted for by an exceptionally fine state of the air?

Mr. Capron's notes may be compared with the altitude of Sun at the time he was observing, thus:—

Date.	Alt. of Sun.	Mr. Capron's remarks.
1878, Sept. 6.	° 35 rising.	Klein's crater unnoticed. Other objects distinct.
1878, Sept. 16.	22 setting.	Crater unseen.
1878, Nov. 2.	22 rising.	Crater seen well as a cup-like cavity with nebulous margin.
1878, Nov. 10.	68 setting.	No detail visible.

It will be seen that the result of Mr. Capron's observations accords with that derived from Dr. Klein's. That Mr. Capron was able to see so well when the sun's altitude was as great as 22° may partly be on account of an exceptionally fine night, but more probably was caused by the greater power and penetration of his telescope. It is worthy of remark that Mr. Capron noted no black nucleus.

That the object is really a *crater* at all seems open to considerable doubt. On 1878, Oct. 17, 10^h to 12^h, I was observing the region while the terminator was passing over it, a condition under which one might reasonably expect that a crater of even less than 3 miles diameter would be easily visible. Yet the result of my careful search was that nothing whatever was visible *of the character assigned by Dr. Klein to his crater.* The definition at the time was not fine, but it was difficult to think a crater such as Klein's existed there without being very visible under such an extremely low angle of illumination. If a crater 3 miles diameter was ever full of deep black shadow it ought then to have been full to the very brim.

The probability that the object is *a shallow depression* combined with a dark ground marking seems very great, and is supported by the difficulties of observing it. Such an object might only be observable under a rising sun, and it should be noted that on every occasion when it was observed by Dr. Klein, as well as Mr. Capron's view, it was under a rising sun, while my own observation was under a setting sun, and then it was invisible. Further support of the idea that its character is as here indicated is derived from Mr. Capron's drawing, plainly showing it as a shallow depression. I have (by the courtesy of Mr. Ellery) a copy of the very beautiful photogram of the Moon, taken with the great Melbourne telescope, dated 1873, Sept. 1. Of this I have made an enlargement of the locality under discussion, which magnifies it to a considerable extent. Upon this there is visible what appears as a shallow depression, with its dark side towards the Sun, which was then rising. Whether this can be taken as a further support of the suggestion as to the true nature of the object I will leave to others to determine, but it certainly accords entirely with Mr. Capron's drawing.

Whether Dr. Klein's supposed crater is really *a new object* it is not easy positively to determine; but surely the probability is much against such a supposition. On the Melbourne photograph above referred to, dated 1873, it is clearly shown, while upon another, by Rutherfurd, of New York, which I possess, dated 1871, it is also visible, although not so decidedly clear as on the Melbourne copy; and I learn from Mr. Christie that at the Greenwich Observatory is another by Rutherfurd, dated 1865, March 6, upon which it is also seen. So we must conclude that whenever it was new it was at some date anterior to 1865.

But whether new or not it must be confessed that the maps do not *exactly* represent the region in many points; and they also differ to considerable extent *inter se*. That is probably accounted for by the fact that observations of the district are most frequently taken about first quarter, and that the locality is one subject to great differences of appearance under the opposite illumination.

As a contribution towards a better knowledge of the appearance of the region under a setting sun I submit a sketch (Plate I.) taken on 1878, Oct. 17, 10^h to 12^h. Its western boundary coincides with the terminator at 11^h. The few objects it contains on the south of Hyginus are there chiefly as guides to the relative positions of the objects on the north of the clefts.

Hyginus itself appeared without a true wall.

Triesnecker a is the crater nearly south of Hyginus.

A very shallow valley runs between the two, which hitherto I have never seen described, and there is no indication of it upon any map with which I am acquainted.

Hyginus b is the small crater nearly east of Hyginus.

A wide and very shallow valley runs northward from Hyginus in a recurved form, and enters a break in the outer wall of the curious formation which is badly described as *Mädler's spiral*. This valley at the time of observation exhibited nothing whatever of the cleft which Dr. Klein has described as being near its position, but was bounded on the east and west by what appeared to me simply as *lines of fault*. It is possible that the *fault* forming the eastern boundary of the valley may appear under a rising sun more like a cleft than at the time of my own observation. But I cannot think Dr. Klein's description of what he saw applies to this shallow valley, which is far wider than what he appears to have seen, if I understand his description rightly.

A series of very low curved banks on the west of this valley were curiously suggestive of waves from Hyginus as a centre of disturbance. Of these I find no traces on the maps.

Between the northern ends of those banks and Mädler's spiral were some *rounded hills* in about the same position as Klein's crater, but there was nothing crater-like about them or near them.

A ground-marking darker than the general tint of the surrounding locality was visible between the hills last mentioned and the spiral; but it was quite a flat tint, without any thing suggestive of hollowness, and across it fell the shadow of the outer wall of the spiral.

Two other ground-markings of similar nature were seen near the western end of the two long ranges of hills which are conspicuous near the bottom of my drawing. The larger one, partly inclosed on all sides by the ends of ranges and isolated hills, appears, in a small telescope, and under particular illumination, not unlike a crater.

The spoon-like termination of the north-east branch of the Hyginus cleft was finely seen, while the complete cleft came out exceedingly well, its southerly side appearing very dark and its northerly side brilliant white.

Southwards from Triesnecker *a* ran the well-known and decided cleft which appears on Neison's map.

The instrument with which my own observations were made was a Newtonian equatoreal, speculum by With, of 8·15 inches aperture, powers 180 (Ramsden) and 270 (Kellner).

Henry Pratt.

Brighton, 1878, Dec. 6.

Since the above was written Mr. Capron has favoured me with the following notes:—"1878, Dec. 4, 4^h 45^m, Moon's age 10^d 8^h. Air unsteady, but definition fairly good. Eyepieces as before. In the place of Klein's crater was a dull spot, darker than surrounding region, fairly well defined, but apparently *flatter* and less cup-shaped and more even in tint than in 1878, Nov. 2. Its margin was sharper and better defined than on that date, and the side most lighted up was distinctly ragged and broken up.

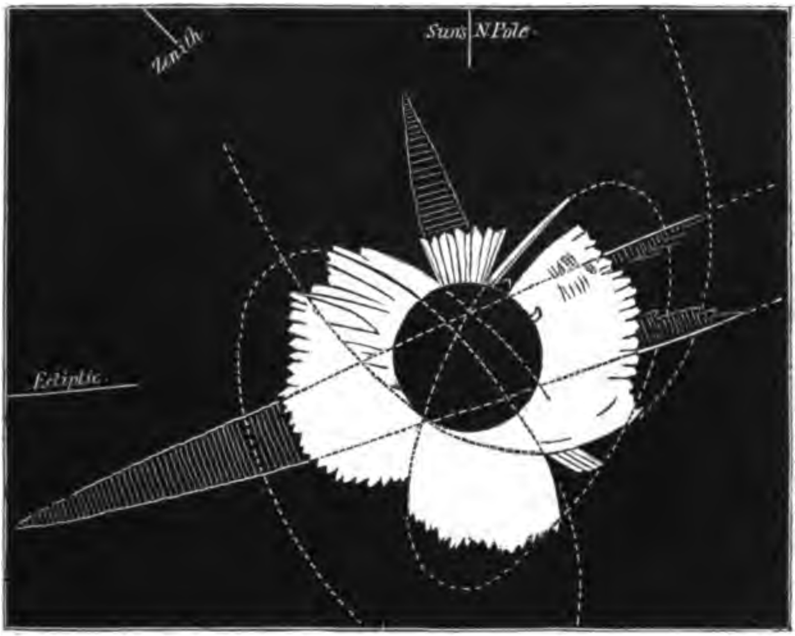
"The most curious feature was that the whole object was apparently considerably larger than on Nov. 2, extending closer to Hyginus on one side and to the bright range on the other."

The Sun's Corona and Meteor-Streams.

In my paper read before the Royal Astronomical Society in November, I endeavoured to account for the appearance of the Sun's Corona, as seen in America during the total eclipse in July last, as being formed chiefly by a combination of meteoric orbits, and I exhibited a diagram to illustrate this view of the subject, which is here reproduced.

The wedge-like distant streamers which are shown in the general direction of the ecliptic and of the Sun's north pole are derived from a sketch and description given me by Prof. Cleveland Abbé, who observed at an elevation of more than 11000 feet, using spectacles only, and who has stated his impression that these streamers were of meteoric origin.

His observations are confirmed by others; I refer particularly to Dr. Edmunds of Denver, and to the representation given in No. 461 of 'Nature' by Mr. Lockyer, who observed the eclipse during part of the time with the naked eye, and described the general appearance as that of a wind-vane.



The firm lines on the white ground in the woodcut are carefully reduced from the original sketch taken by me during totality, being at the time entirely free from any hypothesis respecting the nature of the corona.

The firm lines on the black ground are derived from Prof. Abbé's sketch. They do not extend in the woodcut, however, so far as he observed them. The dotted lines, wherever they occur, are continuations of the hypothetical orbits.

The cone-like streamers of the "wind-vane" suggest a band of considerable breadth, having an extensive orbit of great excentricity, which may be compared in appearance to the apses of Saturn's ring when seen very obliquely. The lines seen in the more central part seem to coincide with orbits of smaller extent, all of them, like the November meteors, having the material part concentrated over a comparatively small part of the orbit. The dusky mottlings in the north-western part of the corona might seem to belong to an orbit seen in section, and with the material part at such a distance from the Sun as to receive but little illumination and therefore intercepting some of the light of the more central corona.

During totality I had recorded that in the direction of the ecliptic the corona faded off imperceptibly both east and west. The streamers of the "wind-vane" seen by the naked eye observers explain the difficulty I had in assigning a definite termination in these directions. I am far from arguing that the whole of the corona is explained on the meteoric theory, and particularly so in the case of the almost radial emanations which appeared like bundles of spun glass proceeding from the Sun's poles, especially the northern.

F. C. PENROSE.

Mrs. Gill's 'Six Months in Ascension.'[*]

ACCOUNTS of scientific expeditions are usually uncommonly dry. Fortunately Mrs. Gill has shown us that one such enterprise, at all events, had its romantic side, and that adventures are to be met with even in the determination of the Solar Parallax. The simple narrative of the privation and toil which she cheerfully underwent in company with her husband affords touching evidence of the enthusiasm which prompted the expedition and carried it through to a successful issue. Of the difficulties encountered, but a slight idea could be gathered from the letters which Mr. Gill communicated to the 'Observatory' last year. The results secured were all-important in his eyes, and the troubles which enhanced their value were merely hinte at. Mrs. Gill has now taken us behind the scenes, and has shown us the machinery which kept every thing going. Nothing but her untiring devotion could have rendered possible that ceaseless watch on the planet Mars, without which the enterprise would have ended in failure.

The origin of the expedition is well told by the author, and its object is unfolded in familiar language by the homely aid of knitting-needles. The sea voyage was painfully devoid of interest, at any rate to Mrs. Gill; but the sight of *terra firma* revives her, and we have a lively description of Madeira, Teneriffe, and St. Helena. The route to Ascension may be described as two steps forward and one back; and the travellers had to go as far as the last-named island in order to meet the steamer from the Cape to Ascension. It was with much regret, doubly intensified on Mrs. Gill's part, that they left the pleasant shores of St. Helena, rich in its astronomical associations, not to mention others of a more mundane character, for the barren isle of Ascension. The difficulties of landing, the anxiety caused by persistent cloud, and the successful though arduous attempt made to evade it, incidentally mentioned in Mr. Gill's letters, are here vividly painted by the hand of an enthusiast. Whilst the astronomer was struggling night after night with the clouds, his wife set out on an exploring expedition over the crushed cinder, by the light of a bulls'-eye lantern, to determine whether the cloud which came up at nightfall extended over the whole of the island. The result of her expedition was, that the south part appeared to be comparatively free from the cloud formed by the Green Mountain, and it was resolved that the observatory should be moved at once. Mr. Gill accordingly pitched his tent on the "clinker," and his wife insisted on joining him. The hardships involved in this change of quarters can only be realized from Mrs. Gill's pages. The only habitable part of the island is in the immediate neighbourhood of George Town or "Garrison," as it is more usually called, and even that could hardly be considered an abode of luxury. The rest is formed of needle-like masses of lava or "clinker," without a particle of moisture or a sign of vegetation. "Imagine," says Mrs. Gill, "the neighbourhood of a great iron-foundry strewn with the accumulated slag of years, some of it in rough compact masses of various sizes, some reduced by the action of time into a fine powder, ready to be stirred into a cloud with every breath of wind. Such was the ground-work upon which one had to make things comfortable at Mars Bay." To add to the discomfort the inhabitants were suffering from a scarcity of water, the consequence of a long-continued drought; and the chaplain flatly refused to pray for rain, saying that it was England's duty to supply the "*Flora* tender" (as Ascension is called in the Navy List) with proper condensers. However, the hardships were cheerfully endured, and despite much anxiety the opposition of Mars was satisfactorily observed. Every reader will sympathize with Mrs. Gill in her enjoyment of a ten days' holiday on the Green Mountain after their toils were over. She seems absolutely to revel in the sight of green things after the "abomination of desolation" of the clinker, where, but for the sea and its inhabitants, nature would have been utterly unlovely. Every thing seems to have a delicious freshness to her as she came from the arid desert of Mars Bay, and her delight in natural beauty is infectious, giving a charm to the description even of the most ordinary objects.

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[*] 'Six Months in Ascension. An Unscientific Account of a Scientific Expedition.' By Mrs. Gill. London: John Murray, 1878.

Life in Ascension is so strange, and the island is so peculiar in its character, that Mrs. Gill's story has an interest of its own, quite apart from its astronomical surroundings. It is written in a lively style, and interspersed with some well-told anecdotes, and the general reader will find plenty to amuse him or her, as the case may be.

To the astronomer it is peculiarly instructive as a faithful record of the troubles which beset scientific expeditions, and of the skill, devotion, and perseverance by which difficulties are conquered. If such accounts had been more frequently written we should probably hear of fewer failures from "unforeseen accidents" when astronomers start without due consideration of the troubles they may have to face. We trust that Mrs. Gill may accompany her husband on many another successful expedition, and may give us many another pleasant narrative, such as that which records her visit to Ascension.

The book is prefaced by an introduction by Mr. Gill, in which the Solar Parallax is treated in a more ceremonious manner than in the pages which follow. The writer gives a sketch of the history of the problem of determining the Sun's distance from the time of Aristarchus, when the Sun was supposed to be nineteen times as far as the Moon, to the late opposition of Mars, the observations of which show him to be nearly 390 times as distant, or some 93,000,000 miles from us. The received value has gone through some curious vicissitudes in the interval.

We must not omit to mention that an excellent map of Ascension, taken from the Admiralty chart, is prefixed to the book, and materially adds to its value. A striking feature in this chart is the mass of craters in the southern part of the island, which to an astronomer must suggest a comparison with the Moon's surface. During the ascent of Green Mountain Mrs. Gill counted *twenty-one* extinct craters spread out at her feet, and this in a small portion of an island which altogether is only seven miles in length and breadth. The whole island in former times was simply one great volcano, and is now a mass of Nature's cinders.

EDITOR.

Double Stars for January.

IN addition to the great binaries between 2^h and 4^h which have already been given, the following pairs, one of which has probably not been measured since 1857, may be observed with advantage to this department of astronomy.

Σ 221. R.A. $2^h\ 3^m{\cdot}1$, Dec. $+19°\ 47'$. Mag. 7·7, 8·9.
1871·78 $144°{\cdot}4$ $8''{\cdot}79$ Talmage.
Probably binary.

Σ 227. R.A. $2^h\ 5^m{\cdot}4$, Dec. $+29°\ 45'$. Mag. 5, 6·4.
Colours, yellow, blue. Probably binary.
1877·07 $76°{\cdot}7$ $3''{\cdot}77$ Hall.

Σ 326. R.A. $2^h\ 48^m.5$, Dec. $+26°\ 24'$. Mag. 7·5, 9·7.

1868·77 216°·8 8″·59 OΣ.

A physical pair.

Σ 355. R.A. $3^h\ 0^m.9$, Dec. $+7°\ 56'$. Mag. 8·7, 9·5.

1857·11 142°·8 2″·65 Secchi.

Probably binary.

Σ 360. R.A. $3^h\ 4^m.5$, Dec. $+36°\ 46'$. Mag. 7·8, 8.

1869·38 139°·1 1″·67 Dembowski.

Probably binary.

Σ 371. R.A. $3^h\ 10^m.4$, Dec. $+46°\ 35'$. Mag. 8·3, 10·3.

1867·45 81°·7 3″·32 Dembowski.

Σ 377. R.A. $3^h\ 13^m.7$, Dec. $+18°\ 45'$. Mag. 8·3, 8·7.

		°	″	
A B	1868·90	120·7	1·03	Dembowski.
$\frac{AB}{2}$ and C	1870·92	220·4	25·15	,,

A B probably binary.

JOSEPH GLEDHILL.

Meteor Notes for January.

MORNING observations may be made on the first three days, and continued after about the 16th. The period from the 12th to the 23rd is suitable for evening watches. The *Quadrantids* (with a centre at 232° +49°, Greg) form the chief shower of the month, and should be looked for on the mornings of the 1st–3rd. They were well observed last year by Prof. Herschel at Hawkhurst, Kent; for on Jan. 2, 4^h–$4^h\ 30^m$ A.M., 20 shooting-stars were seen, of which 17, most of them brilliant, belonged to this stream, and showed a radiant (not very certainly deduced, owing to prevailing haze, which hid many of the fainter stars) at 222° +55° near θ Boötis. A shower of slow, large meteors from a point near γ Eridani may be expected on about the 4th–7th, and there is a radiant at 110° +70° (*p q* Camelopardi) that requires verification during this month. A new shower at 264° +36°, near θ Herculis, was suspected by Corder on the 27th, 1877, and an active display of slow, bright meteors was traced by the writer between the 4th and 17th from a position at 295° +53°. It is difficult to select from the large number of radiants visible in January those which offer the most conspicuous displays, but it is believed the following positions include several of them:—

131°	+48°	175°	+14°	180°	+56°	208°	+27°
201	+54	221	+41	104	+53	145	+ 5
180	+35	230	+30	155	+64	104	+18

Recent Observations.—At Bristol, during the three years 1876–78, the total number of meteors seen by the writer during habitual watches, extending in the aggregate over 429 hours, is 4513, of which 520 were August *Perseïds.* Deducting the latter, and separating the A.M. and P.M. observations, the figures are:—

1936 meteors seen in 161¼ hours A.M.
2057 ,, ,, 267¾ ,, P.M. } 3993 meteors in 429 hours.

About five sixths of the whole were noted during the last half of the years mentioned, when, making certain allowances, the horary numbers were 16·4 A.M. and 10·1 P.M. for one observer. The rates for the first half are considerably less, but the observations are not sufficiently full for this period to give good results. Of the total number seen the paths of 3213 were registered. Their magnitudes were:—

>1st.	1st.	2nd.	3rd.	4th.	5th & less.	Total.
95	218	634	821	1003	442	3213

The average length of path of 3203 of these was 11°·23. Schiaparelli found from 1951 paths recorded by Tupman, 1869–71, an average of 11°·0, while the mean apparent length of meteor-tracks assigned by Coulvier-Gravier is 13°·9. The longest path of all those recorded at Bristol was that of a 2nd mag. meteor on Dec. 13, 1876, $7^h\ 28^m$ P.M., =122°. Course from 135° +66° to 334° +10°; radiant in Leo minor, on the north-east horizon.

A fine meteor was seen on Nov. 18 last at $9^h\ 50^m$ by Mr. Corder at Chelmsford, and by the writer at Bristol. At the former place it was rated = ♀, and shot from 11° −22° to 17° −30°. At Bristol the end-point only was seen from 47° −26° to 50° −27½° = ♃. Prof. Herschel finds the radiant at 354° +1°, and the end-point exactly given by the two accounts, 271 miles distant (horizontally) from both Bristol and Chelmsford, at an altitude of 8½° (observed) at both places. Height 41 miles, and allowing 9 miles for earth's curvature, 50 miles above Saumur (near Tours), on the Loire. The Chelmsford beginning-point, adopting the projected radiant for the track's direction (from alt. 33°, 38° W. from S.) or descending slope to this point of disappearance, is 328 miles (horizontal) distance from Chelmsford, height 83 miles, or (allowing 13 miles for earth's curvature) 96 miles over La Rochelle, on the French coast, at the mouth of Charente. Length of real sloping path 77 miles. The radiant-point appears to be the same as that of a shower of slow, bright meteors, slightly traced at Bristol on Dec. 1–2, 1877, at 350° +2°, including a fireball with a path from 334° −11° to 322° −18½°.

Observations for the *Andromedes* appear to have been attended with purely negative results. Prof. Herschel, at Newcastle-on-Tyne, saw none of them during a watch of 20 minutes on fine evenings of Nov. 23rd–28th. The 29th–31st were cloudy. At Bristol, Chelmsford, Debenham, Crediton, &c. the sky was also overcast on the 27th and 28th, and no meteors could be seen. On the evening of the 26th, at Bristol, there were breaks in the clouds, but no sign of any display. On the 29th, at Chelmsford, Mr. Corder observed 17 meteors between 5.30 and 9.30, of which 6 were *Andromedes*, including two of the 1st mag. near the radiant, which appeared to be about 23° +44°. He noted about 20 *Taurids*, mostly small, in November, and a 1st mag. *Leonid*; but these were the only meteors of importance.

Very few, if any, *Geminids* have been seen this year, for at the period of their annual return, moonlit or cloudy skies (with intensely cold weather) effectually prevented watching.

W. F. Denning.

CORRESPONDENCE.

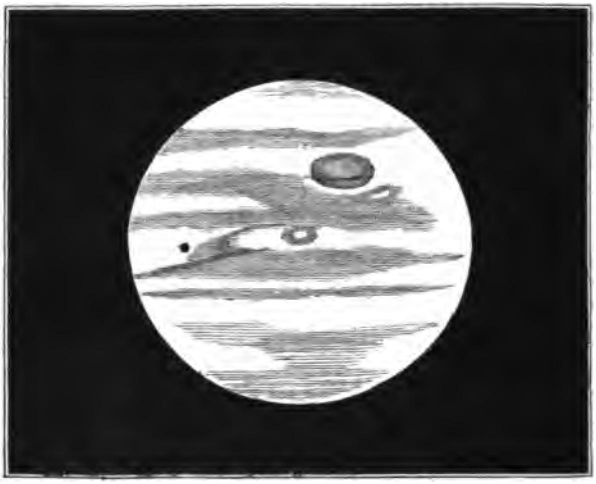
To the Editor of 'The Observatory.'

Markings on Jupiter.

Sir,—

I have carefully watched the astronomical journals to see if any thing unusual was noted by European or American astronomers in the behaviour of Jupiter's belts between the dates of July 6–15. Having seen nothing, I venture to mention some phenomena I saw myself, and to send you a rude drawing to represent them at one stage of their progress.

The appearance of globules and transient spots in the belts of Jupiter is so common that it has ceased to excite interest; but on July 6 I noticed two bright spots, so conspicuous and well-defined that they might easily have been taken for satellites in transit. I made the following note of them in my observing-book:—"1878, July 6, 11^h Gl.M.T. Jupiter presents a splendid appearance. Two very distinct and remarkably round and bright spots seen on the inner edge of the broad southern equatoreal belt, about one fourth the planet's diameter distant from each other. The centre of the planet is *between* them, the *middle* space between the spots nearer by a little to the preceding side. They are nearly of equal size, and entirely similar in appearance." Though this note was made, I should probably have thought no more of the matter but for subsequent phenomena. On July 9 Jupiter was again examined from 11ʰ to 12ʰ 5ᵐ Gl.M.T. I was surprised at the extraordinary changes which had occurred both in the spots themselves and in the contiguous parts of the belts. Two spots still showed, but much enlarged and with irregular outline. Their position, moreover, was very different from the position noted on July 6. The place of one was a little west of the most easterly spot seen on July 6, and near the inner edge of the northern equatoreal belt; but the other had changed to south side of the southern equatoreal belt, and far eastward, and seemed to be rapidly breaking up. What was more remarkable was a total change in the whole structure of the belts in that vicinity, and the formation of an elliptic cloud-like mass, separate from the general contour of the belts. This cloud was almost a perfect oval in shape, and was preeminently rose-tinted. But the most remarkable phase of all was the rapid *proper motion* of this "elliptical cloud." I watched it for nearly one hour, and in that time its front moved on the disk of the planet about one fourth of the planet's diameter. It passed by the most easterly of the spots, and its front had reached to the most westerly spot, and was passing it at 12ʰ 5ᵐ Gl.M.T. At this time exhaustion from previous work and premonitions of neuralgia compelled me most reluctantly to abandon the observations. The drawing shows the phase *inverted*, and was made at the time by my son, a youth of fifteen years. While it is *rough* it is rigidly *true* to phenomena. It represents the phase at 12ʰ 18ᵐ Gl.M.T., corresponding to about 18ʰ 36ᵐ G.M.T. The black dot shows the shadow of Sat. II., which was visible part of the time, but passed off at 11ʰ 7ᵐ. Aperture 12½ inches, focal length 17 feet, power 275.



July 10, 10^h Gl.M.T. An examination of Jupiter showed no trace of the two spots nor of the "elliptical cloud."

July 15, 10^h Gl.M.T. The belts of Jupiter showed a number of round spots, particularly south of the equator. The small disks so numerous and close together as to give the appearance of a curdled mass. One large and prominent spot directly between the northern and southern equatoreal belts.

I am, Sir, yours truly,
C. W. Pritchett.

Morrison Observatory,
November 1878.

P.S. On October 5, at 10^h 32^m Gl.M.T., the shadow of Sat. III. of Jupiter was seen projected on the disk as an ellipse whose major axis was 2¾ times its minor. If this effect is due to the oblique direction of the axis of the shadow to the surface, why does it not show more frequently? I have often observed these transits, but never before noted such a projection.

Transparency of the Atmosphere at High Altitudes.

Sir,—

In the paper of Mr. Penrose on the solar eclipse, in the October Number, he speaks of "the uncommon brilliancy of the stars" in the atmosphere of Colorado, and says they were "visible on all sides down to the horizon, while the Milky Way seemed absolutely clotted with light." This remark recalled to me most vividly one of the most pleasant of all my experiences connected with the recent eclipse, observed on the elevated plains of the West. It was the night of July 30, our last night at Las Animas. The work on the eclipse was done, the tents had been despoiled, and the instruments packed. The day, like the 29th, had been one of the clearest, even in that high region; and the night was one the like of which one may seldom see. For some time I enjoyed it alone; but the views were so surpassingly grand and beautiful that (weary as I knew he was) at last I called out Prof. Eastman; then Dr. Schuster came out, and afterward others, until almost the entire party felt a kindred enthusiasm as they gazed up and around on those starry depths. The Milky Way crossed the meridian near our zenith, and from a point north-east of the Swan, down the grand slope of the south-western sky, there was "a marshalling of the heavenly hosts" on a scale of magnificence and of exhaustless riches which we can never forget. The two contiguous branches of the Milky Way, as Prof. Eastman suggested, resembled distant parallel chains of hills covered with purest snow, while between ran a dark broad belt, like a great winding river flowing on in silent beauty between its snow-crowned cliffs. The analogy was a striking one, and I shall ever carry that picture in my mind. One peculiarity I noted then which had escaped me before. It was the numerous alternations of bright and dark patches in the galaxy. In place of the continuous misty light which *alone* arrests the attention in an ordinary atmosphere, the

"Via Lactea" was broken up into a very diversified succession of bright ridges and star-decked regions, and of void spaces approaching to *blackness* in the contrast. I was surprised at the number of these dark spaces, and the contrast between them and the contiguous bright regions was not unlike that between the bright region surrounding θ Orionis and the dark void east and north-east of it, as seen in the telescope. Two of these dark spaces were preeminently noteworthy. One is just a little north-east of α Cygni, and the other in Sagittarius, east of α Scorpii. The shape of this last is very peculiar, having the appearance of a hilted dagger with the point turned towards Antares. Many of these dark spaces I can now recognize on good nights in our Missouri atmosphere. I happened to remember the remarks of your correspondents concerning the visibility of λ Ursæ Minoris ('Observatory' for February and April). We all looked for the "test-object" to natural vision. It was so *easily* seen by every one as to occasion surprise that it should ever be otherwise. At last Mr. Paul, of the Naval Observatory, surprised the company by announcing that he distinctly saw one of Jupiter's satellites. Several others testified with equal positiveness, and uniformly agreed in its position. An opêra-glass was brought out, and the satellite was seen in the assigned position. Having used a 3½-inch telescope on several nights while in Colorado in examining planets, double stars, clusters, and nebulæ, I am quite confirmed in the conclusion of Dr. Henry Draper (Amer. Journ. Sci.) that the *steadiness of the images* is not at all improved at those high altitudes. While much more may be seen, both in the telescope and by the unaided eye, the quivering or vibration of the images, which is so troublesome in all micrometric measures, is not at all relieved.

I am, Sir, yours truly,
C. W. PRITCHETT.

Morrison Observatory,
November 1878.

The Sun's Corona and Meteor-Streams.

SIR,—

With reference to Mr. Schuster's article in your last number, I cannot quite reconcile two points therein. But before proceeding, I would observe that I write this note more as an inquiry than as a criticism on Mr. Schuster; for I am not sufficiently conversant with the various considerations connected with the subject to venture on the latter.

If I understand aright the quotations from Dr. Draper and Prof. Young which Mr. Schuster gives us, the photographs and observations of the spectrum of the corona in the last solar eclipse tended to show not only that the corona consisted on this occasion more of solid or liquid bodies than of gaseous matter, but also that there were many more solid or liquid bodies in the corona on this occasion than at the times of preceding eclipses—that is, in 1869, 1870, and

1871. Now it is this latter point which I cannot quite make fit with Mr. Schuster's speculation on the cause of Sun-spots. This speculation—the meteoric theory of the cause of the spots—would of course require the presence near the Sun of a much larger number of meteors at a maximum period of spots than at a minimum period. Now the years 1869, 1870, and 1871 were near a maximum period, and 1878 was a minimum one; yet we have, according to the spectrum observations, a *smaller* number of meteors surrounding the Sun in 1869, 1870, and 1871, than in 1878. The question simply is, Have I grounds for supposing that these observations indicate a larger number of meteors near the Sun at the last eclipse than at the former ones? If I have, then I do not very well see how the meteoric theory is to be sustained.

I am, Sir,
Yours faithfully,
JAMES L. M^cCANCE.

Rathfern, Rayners Road,
Putney Hill, S.W.
1878, December 16.

The Eclipses of Jupiter's Second Satellite.

SIR,—

When the planet Jupiter is near quadrature with the Sun it occasionally happens that both the disappearance and the reappearance of the second satellite in the same eclipse are visible from the Earth. Both phases are, however, so seldom seen in the same eclipse, that Sir John Herschel, in his 'Outlines of Astronomy,' 4th ed., 1851, writes thus:—"It is to be observed that, owing to the proximity of the orbits of the first and second satellites to the planet, *both* the immersion and emersion of either of them can never be observed in any single eclipse, the immersion being concealed by the body if the planet be past its opposition, the emersion if not yet arrived at it." Other astronomers have inadvertently made the same remark without any qualification. The 'Nautical Almanac,' however, truly remarks that it is seldom both phenomena can be observed at the same eclipse of the second satellite. Out of curiosity I have examined all the numerous records of eclipses of the second satellite in the pages of the Royal Astronomical Society's 'Monthly Notices' and the 'Astronomische Nachrichten,' from the close of 1860 to the middle of the current year. The result of my examination is that three eclipses only have been observed in both phases during that period. The first was observed at the Lawrence Observatory, U.S.A., on 1875, June 28, the second at Windsor, New South Wales, on 1875, July 20, and the third at Adelaide, South Australia, and Windsor, New South Wales, on 1876, August 21.

I am, Sir, yours faithfully,
JOHN TEBBUTT.

Observatory, Windsor,
New South Wales,
1878, Oct. 23.

NOTES.

Metropolitan Scientific Association.—We have received the following Report. The twelfth session was commenced on October 22nd at the Society's rooms, Ward Schools, Aldersgate Street, E.C., when the President, Mr. Charles Judd, F.R.A.S., delivered the usual introductory Address to an appreciative audience. Mr. Judd commenced with a review of the intellectual system of Comte in relation to Astronomy, and his remark on the necessary limitations of our knowledge of the stars:—"We can conceive the possibility of determining their form, distance, size, and movement, but should never by any method be able to study their nature, chemical composition, and structure." The patient investigation of a few years, however, had enabled observers to solve a problem which Comte had declared insoluble. These observations led the speaker to the subject of Light, and an eloquent exposition was given of the progress made from the earliest to the most recent times, including the great discoveries by Fraunhofer, Kirchhoff, Lockyer, and Huggins. At the close of the Address a cordial vote of thanks was passed, and the meeting adjourned till November 26th. The Metrop. Sci. Assoc. is now permanently located in the city, and numbers amongst its members many gentlemen connected with astronomical science; and papers on this subject are frequently given at the monthly meetings.

The Figure of the Earth*.—The re-measurement of the southern portion of the great Indian arc under Colonel Walker's superintendence and its extension to a length of 24° have furnished Colonel Clarke with data for a fresh determination of the figure of the Earth. He commences by remarking that the fraction which expresses the ellipticity of the Earth has apparently a tendency to increase as the data are added to. Thus Airy and Bessel, from the very imperfect data of forty years back, obtained $\frac{1}{298}$, which, on the completion of the Russian and English arcs in 1858, was replaced by $\frac{1}{294}$, and the recent geodetic work in India a further increase of the fraction, and so an assimilation to that obtained by pendulum observations.

The data on which Colonel Clarke founds his investigation are:—(1) the latitudes of 13 stations in the Russian arc of 25°; (2) those of 15 in the Anglo-French arc of 22° (selected out of 34 English stations, besides the French); (3) those of 14 evenly distributed stations in the Indian arc of 24°; (4) the longitudes of 7 stations in India; and (5) the Cape of Good Hope and Peruvian arc—in all 49 latitude and 7 longitude stations. Treating the equations by the method of least squares, he deduces:—

$$a=20926202\,; \qquad c=20854895\,; \qquad \frac{c}{a}=\frac{292{\cdot}465}{293{\cdot}465}$$

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* Phil. Mag. 1878, August.

a and c being the equatoreal and polar semiaxes (in feet) of the spheroid which most nearly represents the mean figure of the Earth. The influence of each of the three great arcs in determining these semiaxes is as follows :—The northern ten degrees from 60° to 70° of the Russian arc determine $\frac{3}{2}a-\frac{1}{2}c$; the ten degrees in England from 50° to 60° determine a; the ten degrees in France from 40° to 50° determine $\frac{1}{2}a+\frac{1}{2}c$; the ten degrees in India from 10° to 20° determine $-\frac{11}{14}a+\frac{25}{14}c$.

With regard to the Indian observations, Col. Clarke remarks that they are not well represented by this figure. "The southern station of the arc requires a large negative correction of $-3''\cdot 14$, and the northern station a still larger negative correction of $-3''\cdot 55$. Among the longitude stations, there is left at Bombay a westerly deflexion of $4''\cdot 05$, and at Madras an easterly deflexion of $4''\cdot 50$. The longitudes, in fact, require a larger value of a and a larger value of the ellipticity; while the form of the meridian-arc requires a smaller equatoreal radius and a smaller ellipticity. In other words, so far as the observations we have at present to consider indicate, the surface of India does not seem to belong to a spheroid of revolution: if it does, we must admit large deflexions towards the sea at Cape Comorin, at Bombay, and at Madras." Col. Clarke then proceeds to find the form of the Indian arc from the 66 latitude stations on the assumption that its intrinsic equation is of the form

$$\rho = A' + 2B' \cos 2\phi + 2C' \cos 4\phi,$$

where ρ is radius of curvature in latitude ϕ. This curve includes the ellipse as a particular case. The normal distance between the curve thus found and the elliptic meridian which best represents the Earth as a whole is then tabulated, and Col. Clarke arrives at the following results for the local form of the meridian sea-level in India with reference to the mean figure of the Earth :—"Supposing that there is no disturbance of the sea-level at Cape Comorin, then from that point northwards a depression sets in, attaining a maximum of nearly 20 feet at about 14° latitude; thence it diminishes, disappearing at about 21°. An elevation then commences, attaining at 26° about 9 feet; then this elevation diminishes, and becomes a small depression at 32°. This deformation may or may not be due to Himalayan attraction; at any rate we have here an indication that that vast tableland does not produce the disturbance that might *à priori* have been anticipated. This is in accordance with the fact that there is an attraction seaward at Mangalore and Madras, and slightly also at Bombay: and I think we have here a corroboration of Archdeacon Pratt's theory, that where the crust of the Earth is thickest there it is least dense; and where thinnest, as in ocean-beds, there it is most dense."

The Anglo-French arc shows a deformation nearly as great as the Indian. Starting with an elevation of 8 feet in lat. 40°, the elevation increases to 19 feet in lat. 45°, and then gradually diminishes, disappearing about 55°, from which point up to 60° there is a small depression. The Russian arc shows a small depression of some 4 feet in the lower latitudes (48° to 60°), and an elevation increasing up to 4 feet in lat. 70°.

If, instead of assuming the Earth to be a spheroid, it be taken as an ellipsoid, the following are the values of the semiaxes (in feet) found by Col. Clarke from 51 equations :—

$$a=20926629\,; \qquad b=20925105\,; \qquad c=20854477\,;$$

and taking the "ellipticity" as the ratio of the difference of the semiaxes to half their sum, the ellipticities of the two principal meridians are $\frac{1}{289{\cdot}54}$ and $\frac{1}{295{\cdot}77}$.

The greater axis of the equator is in longitude 8° 15′ west of Greenwich in a meridian passing through Ireland and Portugal and in the opposite hemisphere cutting off the north-eastern corner of Asia and passing through the southern island of New Zealand. The meridian containing the smaller diameter of the equator passes through Ceylon on one side and bisects North America on the other. This position of the axis agrees very remarkably with the physical features of the globe—the distribution of land and water on its surface. The ellipsoidal theory represents the Indian longitudes better than the spheroid ; but there is still left at Madras and Mangalore an attraction or disturbance of the plumb-line seawards. In his former investigation (Mem. R.A.S. vol. xxix. 1860) Col. Clarke found for the values of the semiaxes (in feet) :—

$$a=20926432\,; \qquad b=20921548\,; \qquad c=20853898\,;$$

the longer axis of the equator being in long. 14° 23′ East of Greenwich.

THE SOLAR ECLIPSE OF 1875, APRIL 6 *.—This eclipse was remarkable for the attempt which was made to photograph the spectrum of the corona ; and the important results thus obtained are now presented in a Report by Mr. Lockyer and Dr. Schuster. Two modes of attack were adopted, viz., the prismatic camera and the spectroscopic camera. The former was on the principle introduced by Prof. Respighi for viewing star-spectra, and used by him as well as by Mr. Lockyer with effect in the eclipse of 1871. In Prof. Respighi's instrument a prism of small angle was placed in front of the object-glass of his telescope, an arrangement which, for every bright line in the spectrum of the corona, exhibits a corresponding image. In this way, during the eclipse of 1871, Prof. Respighi saw rings representing the spectroscopic images of the hydrogen and "1474 stuff" in the corona, these rings being 5′ or 6′ in breadth. Mr. Lockyer used a simple train of five prisms without collimator or telescope, and saw similar rings, but not more than 2′ in breadth, a circumstance accounted for by the greater

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* Phil. Trans., Part i. 1878.

dispersion and consequent greater loss of light. The prismatic camera used in Siam had an object-glass of 3¾ inches aperture and 5 feet focal length, with a prism of 8° placed in front. Two photographs were taken with it during totality, the first exposed for 1^m and the second for 2^m. On both photographs, three prismatic images of the prominences are shown. The first of these is at the limit of the photographic spectrum, and evidently corresponds to the hydrogen line F. The second is at an angular distance of 3′ 15″ from it, and is presumably due to the hydrogen line near G (H_γ), which, by means of a goniometer, was found to be 3′ from F for the prism used. The approximate wave-length of the third and by far the brightest of the images was found to be 3957, *i. e.* close to H. The results of the recent eclipse leave no doubt that it is actually the H line, and that the photographic light of the prominences is in great part due to calcium, which seems to go up higher than hydrogen.

It is, however, in the spectrum of the corona that the chief interest centres. The lower part of the corona shows a single well-defined edge (the spectroscopic image of the Moon's limb) passing through the second ring of prominences, and therefore probably due to H_γ. The upper part of the corona shows a single image, corresponding in outline to the photograph taken without the prism, and the monochromatic light forming it appears also to be due to the hydrogen line H_γ. In addition to this line spectrum the photographs show strong marks of a continuous spectrum in the lower regions of the corona. By examining the inner limits on both sides of the moon, it is found that this continuous spectrum extends from F to wave-length 3530 and beyond, in the ultra-violet, and by the help of the structure in the corona itself it appears that the continuous spectrum reaches to at least 3′ from the Sun. These are important results, and some exercise of skill and patience must have been required to unravel them from the entangled record on the photographs.

The spectroscopic camera, consisting of a telescope and ordinary spectroscope, unfortunately failed, presumably through the fault of its collimator, which was too long to admit of adjustment to focus. Various accidents of the voyage caused unforeseen delays, and thus no time was left on the arrival of the Expedition to remedy this fatal oversight.

Eight photographs of the corona with exposures of from 2^s to 16^s were taken with a small camera of 13 inches focal length, and these show a rapid increase in the extent of the corona with increase in the exposure. The last photograph having been exposed 16^s, shows an extent of corona exceeding the diameter of the Sun.

There are also a number of drawings of the corona made by the King of Siam and members of his court, from which it seems that there was a great similarity in the corona as observed in Siam in 1875 and in South Africa in 1874. The corona was on both occasions symmetrical about the Sun's axis, and the west side was much more compact, the east side being broken up into what the Siamese called fish-tails. It is unfortunate that no orienting lines are given either to the drawings or photographs, so that it is by no means easy to tell which is the east side and which is the west. A remarkable rift is mentioned by the authors as being shown towards the south in Mr. Shore's drawing and in all the photographs, including those with the prismatic camera; but there is no means of identifying it, no indication being given of which is north and which south.

As regards the brightness of the corona, the inhabitants of Bangkok, who witnessed both the eclipse of 1868 and that of 1875, are unanimous as to the much greater brightness of the corona in 1875, some of them even asserting that the last eclipse was not total. The light in the former eclipse was much softer and pleasant to look at. In the last one it was quite as intense as a bright full moon. The characteristic feature of the eclipse of 1868 was formed by the prominences, and the corona fell more into the background, being much more irregular and less well-defined. In 1875 the prominences could be seen with the naked eye by only a few, and the corona surrounded the moon like a regular and well-defined star. Some observers note that one prominence in 1875 was of a dazzling white and stood out bright from the background, while the others appeared red and dark on a bright ground. The prismatic photographs show that this prominence contained a great quantity of the ultra-violet light (due to calcium).

MR. R. C. JOHNSON'S OBSERVATORY AT BEBINGTON.—In a paper read before the Literary and Philosophical Society of Liverpool, Mr. R. C. Johnson gives a description of an observatory he has lately erected for a nine-inch reflector. The novelty in this building is in the arrangement of the shutters in the revolving drum. They are of large size (3½ feet wide for a dome of 13½ feet diameter) so as to secure a rapid equalization of internal and external temperatures, a point of great importance for the satisfactory performance of a reflector; and in order not unduly to weaken the drum, Mr. Johnson has placed the vertical shutter not in a line with the horizontal one, but at a point 30° from it. In order to cover fully every part of the sky there is a subsidiary shutter 2 feet in depth opening on hinges below the horizontal one. A theoretical inconvenience in this arrangement is that if an object varies in altitude so that a change has to be made from one shutter to the other, the dome has to be turned through the 30° which separates the two shutter-openings; but Mr. Johnson states that in practice this seldom occurs, while after a year's constant use the frame-work continues quite unaltered, and none of the shutters have at any time exhibited any signs of flexure.

THE MEAN PARALLAX OF STARS OF FIRST MAGNITUDE*.—In a report to the 'Astronomische Gesellschaft' Prof. Gyldén gives the results of a provisional discussion on the average parallax of a first-magnitude star. He remarks that the apparent proper motion as well as the magnitude must be taken into account, the former being quite as trustworthy a criterion of a measurable parallax as the apparent brightness, and assumes that the parallax p of a star of nth mag. with apparent motion s may be represented by the formula $p=P\frac{s}{\sigma_n M}$, where σ_n is the mean apparent motion of a star of nth mag. and M_n its relative distance estimated according to its brightness. P is a constant which for $M_1=1$ represents the mean parallax of a first-magnitude star.

The following table gives the magnitude, parallax, and proper motion for sixteen stars, on which Prof. Gyldén has based his investigation:—

	Mag.	*p.*	*s.*
		"	"
α Centauri	1	0·900	3·674
61 Cygni	5	0·511	5·221
Lalande 21185	7	0·501	4·734
Groombr. 34	8	0·307	2·801
Lalande 21258	8·5	0·260	4·403
Oeltz. Arg. 17415	9·5	0·247	1·200
σ Draconis	5	0·222	1·925
Sirius	1	0·193	1·252
70 Ophiuchi	4	0·162	1·108
α Lyræ	1	0·153	0·349
Groombr. 1830	7	0·147	7·053
ι Ursæ Maj.	3	0·133	0·525
α Boötis	1	0·127	2·258
γ Draconis	2	0·092	0·063
α Aurigæ	1	0·046	0·438
α Ursæ Min.	2	0·046	0·045

Each star gives an equation from which P, the mean parallax of a first-magnitude star, can be determined. Giving equal weight to each one of the 16 equations, P=0″·048. If, on the other hand, several normal equations be formed by taking the simple mean of various groups of the 16 equations, and the sum be taken, P=0″·062. But this result is greatly influenced by a few 1st mag. stars with large proper motion: omitting therefore α Centauri, α Boötis, and Sirius, the remaining 13 stars give P=0″·086. If all 1st mag. stars be omitted, we have P=0″·083 from 11 out of these 13 stars. On the other hand by omitting all stars with proper motion exceeding 2″, Prof. Gyldén obtains P=0″·084 from the remaining 9.

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* Vierteljahrsschrift der Astron. Gesellsch.

The near agreement of the two values in which the extreme cases of brightness and motion respectively have been avoided, gives, in Prof. Gyldén's opinion, some support for the view that the relation beween parallax, apparent motion, and apparent brightness, expressed by the above formula, may be taken as an approximation to the truth.

Prof. Gyldén points out, in conclusion, that Prof. C. A. F. Peters found for the mean parallax of a 1st mag. star $0''\cdot102 \pm 0''\cdot026$, from W. Struve's observations with the Dorpat transit.

AN ACCURATE METHOD OF DETERMINING RATIO OF DISPERSIONS FOR OBJECTIVES [*]—For the construction of an achromatic object-glass it is necessary that the focal lengths of the crown and flint should be in the ratio of their dispersive powers, and the determination of this ratio with the requisite degree of accuracy has hitherto been a matter of some practical difficulty. In the usual process three constants have to be determined, viz. the refractive indices for three lines in the spectrum, whereas only one constant is really required, viz. the ratio of the dispersions at the brightest part of the spectrum, a condition which Prof. Stokes has shown, in the 'Report of the British Association for 1855,' will in practice give the best results. Under these circumstances Prof. Stokes now describes a method depending on the achromatizing of one prism by another, to which he was led in the course of his examination of a variety of glasses which were too much striated to show any of the dark lines of the spectrum. The secondary tints, which change with extreme rapidity when the refracting-angle or the position in azimuth of one of the mutually achromatizing prisms is altered, afford a delicate test, the best result for achromatism being obtained when the secondary colour is a yellowish green. In practice Prof. Stokes finds it most convenient to make the prisms to be compared achromatize successively one and the same prism chosen arbitrarily. In his arrangement a wide slit is placed at the principal focus of a collimating lens and viewed through the standard prism and one of the pair to be compared by means of a telescope. The second prism placed at the centre of a graduated circle is then turned in azimuth till the secondary colour at one edge of the slit is intermediate between yellow and green. The angle of incidence of the rays for this azimuth may be easily determined by reading off the angle between the reflected ray and the incident ray by the help of a small telescope attached to the verniers of the circle, the slit being illuminated with the sodium flame, which nearly corresponds in refrangibility to the brightest part of the spectrum. Prof. Stokes also gives another method depending on the determination of the two azimuths (one on each side of the position of minimum deviation) which give equal deviations.

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[*] Proc. Roy. Soc., No. 188, 1878.

THE ORIGIN OF COMETS*.—Prof. H. A. Newton has made a careful discussion of the distribution of cometary orbits, and the manner in which that distribution may have been modified by the disturbing influence of Jupiter, with the view of deciding whether comets were formed from the condensing solar nebula, or were made from the matter scattered through the stellar spaces. The former hypothesis was put forward by Kant, whilst Laplace originated the latter. Even on Kant's theory the origin must have been remote from the Sun, since comets pass to a great distance from the Sun, in orbits not sensibly differing from parabolas.

Prof. Newton begins by pointing out that a comet to be visible to us must in general come nearer the Sun than the planet Mars. Thus, if we consider a large number of comets shot from a distant point A, and draw a circle through the Sun (S) in a plane perpendicular to AS and with radius twice that of the orbit of Mars, we may look upon this circle as a target, and only those comets which hit it will be visible to us, since their perihelion distance (being half the distance from S at which they strike the target) will be less than the distance of Mars. The initial velocities perpendicular to A S must have been, in the case of such comets, very small as compared with the parabolic velocity. Thus if the masses were parts of the original solar nebula, their normal motion should be in circles; but for some reason they never had much motion perpendicular to A S, or else have lost most of it. The small residuals left by larger opposing velocities might as easily take any direction whatever normal to A S, from which Prof. Newton draws the conclusion that the distribution of the inclinations of the orbits through the two right angles should be uniform. This is on the supposition that A S is large as compared with the imaginary target, and would not apply if the comets came, for example, from the region between Mars and Jupiter. If, however, they came from the distant parts of the solar nebula, the inclinations of the orbits should have been originally distributed uniformly, and the aphelia of their orbits being the points of origin, should all have been near the ecliptic. But if comets are made from the matter of the stellar spaces, only those whose motions are very nearly equal to the Sun's motion can come within sight of the earth. The small residuals that represent the relative motions must therefore, by reason of their smallness, be nearly independent of original absolute motions, and hence the points of origin should be equally distributed over the heavens. Also the poles of the planes of the orbits will be uniformly distributed for any direction of the line A S; and since the latter directions are uniformly distributed, the poles also will be uniformly distributed over the heavens. The inclination of any orbit to the ecliptic will be the distance of its pole from the pole of the ecliptic. Dividing the celestial sphere into zones 10° wide by parallels of latitude, the numbers of the orbits whose inclinations are included in the successive decades of degrees will be as the areas of the zones, *i. e.* as the sines of 5°, 10°, 15°, &c. Thus Prof. Newton concludes that if the comets come from the stellar spaces, their original orbits should have been so distributed that the numbers of orbits whose inclinations to the ecliptic fall in the successive degrees from 0° to 180° should have been proportional to the sines of inclinations. The distribution of aphelia would have been uniform over the heavens, hence their relative frequency at different latitudes would have been as the cosines of the latitudes. The present distribution of aphelia very nearly follows this law, but this in itself is not critical between the two hypotheses. For progression of the line of apsides resulting from pertubations would in time distribute the aphelia pretty uniformly over the heavens, if either hypothesis be true. One conclusion is drawn from this by Prof. Newton, viz.:—that if Kant's hypothesis be true, the period of past time since the comets were aggregated and made to describe these long orbits has been a very great one, and the process of disintegration of comets is a very slow one. These facts, so far as they have force, favour the foreign origin of comets.

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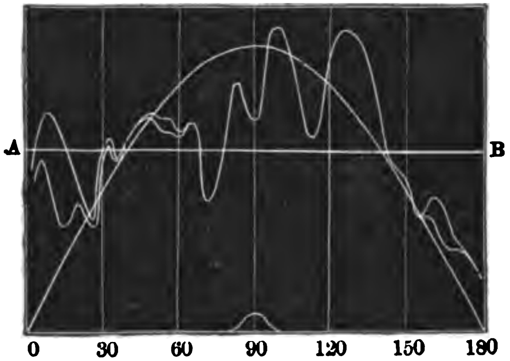
* Amer. Journ., 1878, Sept.

Prof. Newton has tabulated the inclinations of all known orbits and the results are exhibited in the diagram. The abscissæ represent inclinations from 0° to 90° and the ordinates their relative frequency. The regular curve is the curve of sines, which would be the distribution on Laplace's hypothesis, and the straight line A B that on Kant's. The upper irregular curve shows the actual distribution of inclinations, and the lower irregular curve that when the periodic comets (13 in number) are excluded. Instead of following the usual method of dividing the 180° into suitable divisions, and counting the number of comets in each division, Prof. Newton has, in forming these curves, made use of another method, which he believes to be well adapted to many similar discussions. Each orbit is represented by the area of a probability curve $y=ce^{-h^2x^2}$ of suitable parameter, and $h=0{\cdot}2$, which makes the average re-



moval of the area from the central ordinate less than 3°. The area for one orbit at an inclination of 90° is represented at the bottom of the figure. A similar area is assigned to each one of the 247 orbits, and is supposed to be placed centrally on the ordinate corresponding to its inclination. The total ordinate for any abscissa is the sum of the corresponding ordinates of these several small areas.

The smaller irregularities of the curve may be considered to be due to chance; but there are certain large differences between the fact and each hypothesis, which show either systematic action of perturbations, or else that neither hypothesis as stated is exclusively true. Prof. Newton has not been able to establish the action of any principle of selection—any cause which would make it easier to discover comets of small inclination or of direct motion—and he therefore considers the possible effect of perturbations. From an investigation of these in various cases of approach to a planet, he concludes that when a comet loses velocity by such an approach, the inclination of the orbit is in general increased. Now if comets are from stellar space, they come towards the planets at first a trifle faster than if moving in a parabola, and if one of them pass in front of a large planet within a moderate distance of it, it loses velocity enough to remain a permanent member of our system. Most observed comets have on this hypothesis thus lost velocity, and would therefore have their inclinations increased. This is particularly true for inclinations between 45° and 135°, for the corresponding comets are more likely to pass directly across in front of the planets. Hence, on Laplace's hypothesis, perturbations would increase the ordinates between 90° and 135° at the expense of those between 45° and 90°. Again, the periodic comets form a marked group, and should probably be treated separately; and further it is, according to Prof. Newton, reasonable to suppose that a large part of the area lying below the lower curve is due also to comets of short period. For of the 26 comets with inclinations less than 20°, 9 are noted as periodic, 7 have elliptic orbits mostly of short period, though they have not been certainly detected at any return, and of the other 10 about half were not well enough observed to enable us to say whether their periods were short or long.

On correcting the curve by striking off the surplus area below 20°, and bringing back some of the area from the second into the first quadrant to allow for the effect of perturbations, the result corresponds well with the law of sines; and Prof. Newton concludes that the curve of fact does agree well with the hypothesis of Laplace if reasonable allowance be made for known perturbations and for the comets of short period. Further, it appears that the curve of fact cannot be made to agree with Kant's hypothesis by simple and reasonable allowance for perturbations.

The group of comets of short period may, for aught that appears in this discussion, have come either from material of the solar nebula or from outside. In the former case these comets would be merely asteroids gone astray; in the latter there is a reason for their direct motion in the fact that only comets leaving the neighbourhood of Jupiter's orbit with a small velocity can move in these short periods, and only comets overtaking Jupiter can have their velocities so much diminished in a single approach to the planet. It is to be considered, however, that a comet leaving the neighbourhood of a large planet has a peculiar tendency to come under its influence at subsequent revolutions, and a slower velocity of approach changes much the problem at the second passage near the planet.

How the comets first became solid is a question which Prof. Newton leaves to the mineralogist and physicist. That they are solid is, he considers, evident from the fragments broken off them, *i. e.* the meteorites and meteoroids.

THE B LINE IN THE SOLAR SPECTRUM*.—Prof. Piazzi Smyth has examined this striking band under favourable circumstances in the spectrum of a high Sun at Lisbon, and has made a careful map of its constituent lines. From this it appears that the published representations of the group are very inadequate, though an exception is to be made in favour of Ångström's normal solar spectrum. Prof. Piazzi Smyth, with prism-dispersions of 28° to 50° (A to H), and a magnifying-power of 20 on the viewing-telescope, finds that each of the usually seen thick lines of the "preliminary band" is a double line, each line being almost infinitely fine, sharp, clear, and well-defined, whilst the "attached band," which Ångström represents by a telluric shading and four single separate lines, is composed of a set of exquisitely close doubles, followed by some stronger single lines. The B line itself, seen under these circumstances, is no longer a single thick line, but a wonderful grouping of the finest possible lines. Prof. Piazzi Smyth considers that, as the group has such a decided and well-marked physiognomy, it is dependent on one element or elementary combination, and is not the result of a chance coming together of stray lines from all sorts of elements scattered through the rest of the spectrum. He therefore gives a record of micrometer-measures and a graphical representation, in the hope that chemists may find a substance which, under *some* temperature or pressure, may present just such a picture, but in bright lines.

SCHMIDT'S NOVA CYGNI*.—It will be remembered that the bright-line spectrum of this star (which blazed forth in November 1876) underwent important changes as the light waned, and that in September 1877 Lord Lindsay announced that it was reduced to a single bright line, corresponding in position to the characteristic line of gaseous nebulæ. Dr. Vogel, while confirming the fact that the light was monochromatic, disputed the conclusion that the star had become a planetary nebula. The wave-lengths of the three nebular lines are 5003, 4957, and 4861, of which the second is the most characteristic; and, according to Dr. Vogel, the wave-length of the stellar line (γ) was 4990 ± 10, whilst Lord Lindsay's determination gave 4986. Mr. Backhouse's observations, now published, throw some light on this point. They extend from 1876, Dec. 27, to 1878, Feb. 2. Mr. Backhouse observed eight bright lines (two of these only suspected), and compared their positions with those of various chemical elements. It is remarkable that he is the only observer who records a line (δ) at 4960 (by comparison with other spectra) or 4943 (by drawings), which agrees fairly in position with the second nebular line. This line was at first brighter than γ, and second only to H_β; but it rapidly faded out, being only suspected on 1877, Jan. 19. The wave-length of γ, the line to which the spectrum was ultimately reduced, was found by Mr. Backhouse to be 5022, and the mean of all determinations by various observers gives 5008, a result which agrees very closely with the position of the first nebular line. Mr. Backhouse's table of the relative brightness of the several lines from 1876, Dec. 27, to 1877, May 15, shows clearly the progress to monochromatic light. Taking the brightness of γ as 1·0 throughout, H_β was 2·5 at the first date, and had sunk to ·08 by 1877, May 15; and other lines had faded relatively to γ in much the same proportion. The conspicuous band in the yellow appears to correspond fairly with D_3, its wave-length, according to Mr. Backhouse, being 5831, whilst Cornu gave 5880 and Vogel 5800.

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* Monthly Notices, 1878, November.

Satellite of Neptune.

MAJOR and minor semiaxes of apparent orbit on Jan. 30, $16''\cdot4$ and $5''\cdot4$. The satellite is near its greatest *n. f.* elongation in pos.-angle $40^\circ\cdot3$, and near its greatest *s. p.* elongation in pos.-angle $220^\circ\cdot3$, at the following times:—

Jan. 16	4^h	*s.p.*	Jan. 25	0^h	*n.f.*	Feb. 2	19^h	*s.p.*	Feb. 11	15^h	*n.f.*
19	3	*n.f.*	27	22	*s.p.*	5	18	*n.f.*	14	13	*s.p.*
22	1	*s.p.*	30	21	*n.f.*	8	16	*s.p.*			

A. M.

Astronomical Memoranda, 1879, *Jan.* 15.–*Feb.* 14.

Sun. Jan. 15, sets $4^h\ 17^m$, rises $20^h\ 1^m$; Feb. 14, sets $5^h\ 10^m$, rises $19^h\ 17^m$.

Equation of time:—Sun *after* clock, Jan. 15, $9^m\ 37^s$; Jan. 25, $12^m\ 33^s$; Feb. 4, $14^m\ 10^s$; Feb. 14, $14^m\ 25^s$.

Sidereal Time at Mean Noon:—Jan. 15, $19^h\ 38^m\cdot3$; Jan. 25, $20^h\ 17^m\cdot7$; Feb. 4, $20^h\ 57^m\cdot1$; Feb. 14, $21^h\ 36^m\cdot5$.

An annular Eclipse of the Sun, invisible at Greenwich, occurs on Jan. 21–22. The shadow-path passes over South Africa and South America.

Moon. New, Jan. 22, 0^h; First Quarter, Jan. 30, 0^h; Full, Feb. 6, $13\frac{1}{2}^h$; Last Quarter, Feb. 13, 7^h.

Occultations:—Jan. 16, $16^h\ 31^m$ B. A. C. 4984 Oc. R. 301°; Jan. 26, $5^h 11^m$ λ Piscium Oc. D. 68°, $5^h\ 53^m$ λ Piscium Oc. R. 359°; Jan. 27, $6^h\ 4^m$ 45 Piscium Oc. D. 189°; Jan. 31, Pleiades, $11^h\ 28^m$ 16 Tauri Oc. D. 93°, $11^h\ 39^m$ 17 Tauri Oc. D. 48°, $11^h\ 50^m$ 19 Tauri Oc. D. 136°, $12^h\ 0^m$ 20 Tauri Oc. D. 107°, $12^h\ 23^m$ 17 Tauri Oc. R. 321°, $13^h\ 1^m$ 20 Tauri Oc. R. 261°, $13^h\ 2^m$ η Tauri Oc. D. 27°, $13^h\ 25^m$ η Tauri Oc. R. 341°, $13^h\ 45^m$ 28 Tauri Oc. D. 32°; Feb. 4, $7^h\ 35^m$ 58 Gemin. Oc. D. 18°; Feb. 5, $13^h\ 51^m$ θ Cancri Oc. D. 44°; Feb. 7, $16^h\ 27^m$ 43 Leonis Oc. R. 215°; Feb. 8, $14^h\ 55^m$ p^5 Leonis Oc. D. 6°, $15^h\ 41^m$ p^5 Leonis Oc. R. 284°; Feb. 13, $16^h\ 15^m$ 4 Scorpii Oc. R. 313°, $16^h\ 56^m$ π Scorpii Oc. D. 101°, $17^h\ 48^m$ π Scorpii Oc. R. 228°. The angles at disappearance (D.) and reappearance (R.) are reckoned from the *apparent* N. point towards the right hand round the Moon's circumference as seen in an inverting telescope.

Mercury is a morning star, at greatest elongation W. (24° 0′) on Jan. 16, when he rises in the S.E. at $18^h\ 24^m$, or $1^h\ 37^m$ before the Sun.

Venus is an evening star, setting 49^m after the Sun on Jan. 15, and $1^h\ 32^m$ after on Feb. 14. Her disk is gibbous, the illuminated portion being 0·987 on Jan. 15, and 0·959 on Feb. 14, and the corresponding diameters 10″·0 and 10″·3.

Mars is a morning star in the S.E., advancing through Ophiuchus.
Jan. 15, R.A. $16^h\ 56^m{\cdot}6$, Dec. 22° 43′ S., tr. $21^h\ 18^m$, rises $17^h\ 19^m$.
Feb. 14, R.A. 18 29 ·3, Dec. 23 44 S., tr. 20 52 , rises 17 1 .

Jupiter is in conjunction with the Sun on Feb. 8.

Saturn is an evening star, midway between α Pegasi and β Ceti.
Jan. 15, R.A. $23^h\ 56^m{\cdot}4$, Dec. 2° 51′ S., tr. $4^h\ 18^m$, sets $10^h\ 7^m$.
Feb. 14, R.A. 0 6 ·8, Dec. 1 39 S., tr. 2 30 , sets 8 25 .

	Outer Ring.		Inner Ring.		Ball.
	Maj. Axis.	Min. Axis.	Maj. Axis.	Min. Axis.	Diam.
Jan. 20	37″·58	1″·98	24″·99	1″·32	15″·0
Feb. 9	36″·66	2″·49	24″·38	1″·66	14″·7

The south side of the rings is visible, the elevation of the Earth above their plane being 2° 59′ S. and 3° 54′ S. on Jan. 20 and Feb. 9 respectively, and of the Sun 5° 13′ S. and 5° 30′ S.

Uranus is a morning star, and may be picked up about ⅓ of the way from ρ Leonis to Regulus.

Neptune is an evening star, stationary between α Ceti and β Arietis, as last month. EDITOR.

Publications received:—Simon Newcomb, *Researches on the Motion of the Moon*, part i. (Washington, 1878)—Col. A. R. Clarke, *On the Figure of the Earth* (Phil. Mag. 1878, Aug.)—Jacob Ennis, *Electric Constitution of our Solar System, Elements of Sidereal Astronomy* (Proc. Philadelphia Acad. 1878)—*Memorie degli Spettroscopisti Italiani* to 1878, Oct.—*The Analyst, a Journal of Pure and Applied Mathematics* to 1878, Nov. (Des Moines, Iowa)—*Kansas City Review of Science and Industry* to 1878, Nov. (Kansas City, 1878)—*Popular Science Monthly*, 1878, Dec. (Appleton, New York)—*American Journal of Science and Arts* to 1878, Dec. (Dana, Newhaven, U.S.)—*Science Observer* to 1878, Nov. (Boston Amateur Scientific Society)—Camille Flammarion, *Catalogue des Etoiles doubles et multiples en mouvement relatif certain* (Gauthier-Villars, Paris, 1878)—*Etudes et Lectures sur l'Astronomie*, Tome viii. (Gauthier-Villars, Paris, 1878)—J. L. E. Dreyer, M.A., *Notes on the Physical Appearance of the Planet Mars as seen with the Three-foot Reflector at Parsonstown, during the Opposition of* 1877 (Trans. Roy. Dublin Soc. 1878, Nov.)—Prof. C. A. Young *The Recent Solar Eclipse* (Princeton Review, New Jersey, U.S.). EDITOR.

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THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 22. FEBRUARY 1. 1879.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1879, January 10.

LORD LINDSAY, F.R.S. &c., *President*, in the Chair.

Secretaries: Mr. J. W. L. GLAISHER, F.R.S., and
Mr. A. COWPER RANYARD.

MR. GLAISHER reported that 47 presents had been received during the last month, including 14 very beautiful sketches of the planet Jupiter made during the present opposition by Mr. G. D. Hirst, of Sydney; and thanks were voted to the donors.

Mr. Dunkin read a paper by the Astronomer Royal on Observations of Occultations of Stars by the Moon, and on the Phenomena of Jupiter's Satellites made at the Royal Observatory, Greenwich, during the last year. He remarked that only seven occultations had been observed on account of bad weather, but that a long series of phenomena of Jupiter's satellites had been secured, and mentioned that in one instance, the transit-egress of the 4th satellite, the phenomenon took place more than 20$^{\mathrm{m}}$ before the predicted time.

Mr. Dunkin continued:—The second paper is merely a note by myself on what appears to me to be a very important subject, which is this:—You will remember that in the discussion which followed the reading of the Astronomer Royal's paper on the Conjunction of Mars and Saturn at the last meeting of the Society, some reference was made to the very large errors of Bouvard's tables of Saturn deduced from a single comparison of the corresponding tabular places in the Nautical Almanacks for December 32nd, 1879, and January 1st, 1880. That difference it appeared to me—and at the last meeting I expressed my opinion on this point—was much larger than what we find from actual observation. As a matter of curiosity and information to myself I have collected all the observed tabular errors of Saturn during the last eleven years, so as to see the exact rate at which the error is progressing. Every one knows that the tabular error varies in different parts of the orbit of the planet, but that variation should progress from year to year with some degree of regularity. I have put down, in the form of a table, the differences between the observed and the tabular places of the planet Saturn as derived from computations deduced from Bouvard's tables and from observation. The numbers have been deduced from the Greenwich Observations when Saturn passed the meridian at about 6 hours mean time, which is comparable with the time when the planet passes at the end of the year, when the comparison of the corresponding places deduced from Bouvard's and Le Verrier's tables is made.

The observed error of Bouvard's tables in 1872 is 5″·5; in 1873, 6″·1; in 1874, 6″·9; in 1875, 9″·1; in 1876, 9″·7; in 1877, 11″·2; in 1878, 11″·5. In 1879, from a comparison of Bouvard's tables as given in the 'Nautical Almanack' for the last day of this year, and of Le Verrier's tables for the first day of the following year, we have 21″·9. You see there is a general progress of the observed errors from year to year, but not equal to that indicated by the new tables. I also give the errors at the time of opposition:—in 1872, 10″·0; 1873, 11″·1; 1874, 9″·6; 1875, 13″·2; 1876, 13″·5; 1877, 12″·8; 1878, 14″·1.

It only requires a glance at these numbers to show that we have reason to doubt the accuracy of the reputed large error of Bouvard's tables of Saturn at the end of the present year; and may we not safely assume that a portion of the difference, 21″·9, is due to an outstanding error still existing in the new tables?

Mr. Gill. Between his tables of Saturn and the observations of Greenwich, Le Verrier found for certain years an irreconcilable difference of about 4″ or 5″, and this was confirmed by the Paris observations. After the fullest consideration of the matter Le Verrier concluded that either the law of gravitation was wrong, or the observations at Paris and at Greenwich were both wrong; and he came to the conclusion that owing to the complications which arise from the presence of Saturn's ring and the difficulty of observing the limb of the ball, it was more probable that the meridian observations were wrong rather than his tables. I hardly think that so very large a discordance between actuality and observation is likely to arise in meridian observations. It is to be regretted that in so important a matter some independent method of determining the error of Saturn has not been employed, such as heliometric comparison between the planet and stars which would be independent of any such error, because the limb of the planet, or the centre, or the borders of the ring could be compared with stars without a possible error larger than the fraction of a second of arc.

Mr. Neison. Le Verrier entirely omitted to take into consideration a particular class of terms of long period in the motion of both Jupiter and Saturn, on the ground that they would be insensible; and for the same reason he omitted the similar class of terms in the theory of Mars. It has been since shown that these terms, which Le Verrier neglected in his theory of Mars, are not insensible, and it is quite possible that the similar terms in the theory of Saturn may also be sensible. Therefore there may be terms in the theories of the larger planets producing sensible inequalities of comparatively short period which have never yet been calculated.

Mr. Neison. I am about to bring before the Society a subject of perhaps more general interest than the general run of my communications. At the last meeting Lord Lindsay read a paper in which he drew attention to some sudden changes which take place in the appearance of the lunar surface when seen under a very low illumination. In the report of his paper it is stated that Lord Lindsay showed drawings "by way of caution to those observers who seemed inclined to consider that because a crater or lunar marking had not been noted before it must, therefore, be a new formation." Lord Lindsay also stated that some of the markings shown upon the drawings appeared and disappeared in a period of twenty minutes. I think this is hardly a correct interpretation of what astronomers call a new formation on the Moon. Because they happen to see something which they had never seen before they do not think that is sufficient ground for calling it a new formation. Again, were they to see a slight depression in the surface become filled with shadow when near the terminator and so look like a black crateriform object, they would certainly not regard that as a sudden real change on the lunar surface. I think, therefore, that Lord Lindsay's remarks, at any rate as reported, give a very false impression of the nature of the changes which selenographers are inclined to regard as probably arising from real alterations in the moon.

I will place upon the blackboard this drawing of the region around the lunar crater Hyginus, showing its appearance twelve hours before sunset. You will see the drawing shows a considerable number of small craters, hills, and ridges, besides some larger objects, and it was made from my observations in 1874-75. In this region, and particularly to the south of Hyginus, there are a number of low flat ridges with very gentle slopes, and though only thirty or forty feet high when near the terminator they cast shadows of very great length, so that at times, for a very short period, the whole of the surface between a network of these ridges will be immersed in shadow and only the ridges remain visible. A person looking at a region like this would see apparently a great black crater swiftly make its appearance and then rapidly disappear on the night side of the Moon; whereas one accustomed to observe the lunar surface would at once draw the correct inference that it was merely a dimple which existed. Near the terminator the shadows produce great alterations in the appearance of most objects; but these are thoroughly understood, and no selenographer would dream of looking on these as real changes. Yet Lord Lindsay's remarks might lead one to suppose otherwise.

This region around Hyginus has been very frequently observed by the early selenographers, and drawn perhaps 100 times in all, by Schröter, Gruithuisen, Lohrmann, Mädler, and Schmidt. They have all been intent on detecting and delineating every crater that is to be seen; and on the whole their drawings are in fair accord, those with the finer telescopes showing naturally rather more detail than the others. A year ago Dr. Klein, a German astronomer, saw a great black crater in the midst of this region. Schmidt, of Athens, who, in all his observations prior to 1876, had not seen a trace of such an object, on looking in 1877 instantly sees it as a most conspicuous object. I had observed this region with great care in 1874–75, but I never saw a trace of this great black crater which, according to Dr. Klein, now exists in this region. I came, therefore, to the conclusion that if there really did exist in this place a great big deep black crater, I could not possibly have overlooked it, so that it must have appeared since the period of my observations. Is this a new crater? At present I cannot say. There are, however, some astronomers who can. Some say it is not new, for it has always existed, and believe they can see it in Rutherfurd's photograph of 1865; others say it is not new, for it does not exist at all. I put now on the drawing this great black crater which, it is stated, now exists. Is it likely that I could have seen all these very small hills, craters, and ridges, and not have seen this enormous black crater in their very midst? You see it is far too large and conspicuous an object to be overlooked if it exists. If there does exist an object here which is not new, it certainly cannot be a great deep black crater such as has been described by Dr. Klein, for that object could not have been overlooked by the earlier selenographers.

The reason why Mr. Pratt and Lord Lindsay did not see the crater on the 17th Oct. must have been that it was then within the shadow of the lofty ridge on the east. For the same reason I did not see it on August 19th. Others, however, have seen it, and there can be no doubt now that a crateriform object does exist; whether it is a conspicuous deep black crater—the important point—remains to be proved. Some astronomers profess to be able to detect the crater on the Rutherfurd photograph. Those most familiar with the region, and including Dr. Klein, cannot do so, though they can see some dark markings of known origin near the place of the crater. The detection of a marking on the photograph proves nothing; the real question is, does there now exist in this region a large conspicuous deep black crater visible for 24 hours after sunrise? All I can now say is that such a crater was not there when I saw that part of the moon.

Mr. Common. Did you observe the region at sunrise, when the crater would not be in shadow?

Mr. Neison. Yes, in 1875; but then the crater was not there.

Mr. Common. Did it alter in position as well as alter in size? Because I have seen several different diagrams showing the crater nearer these two small craters in Hyginus. I was always given to understand that it was halfway between Hyginus and the Spiral mountain.

A Fellow. I should like to ask whether the range in the middle of that peculiar mountain is higher than the semicircular ridges which surround it. If it should turn out that the central range is loftier, I think that may be an explanation of the difficulty.

Dr. De La Rue. I would ask whether in the photograph which is on the wall that crater is visible. There may be a possible explanation of the object being visible in the photograph and not by the eye. At the time of the eclipse in 1860 there was one very high prominence, "the boomerang," which was perfectly invisible to all observers and yet was photographed by me. A slight difference of tint may be strongly delineated by the actinic ray, and still not be visible to the human eye. Eyes differ as to their sensibility to particular rays, so that one observer may see objects that another does not. Consequently it is necessary to take into consideration all the possible contingencies which may affect observations.

Mr. Christie. I have not had the advantage of studying this part of the Moon under favourable condition of illumination, but I have compared some of the drawings with the photograph. These four craters which are depicted by Mr. Neison seem to agree very well in position with the four spots shown in Mr. Rutherfurd's photograph, and also in the Melbourne photograph; but when we come to compare Schmidt's map of the Moon, Lohrmann's map, and Dr. Klein's drawing of that region, it is very difficult to identify the object. The quadrilateral, which is nearly a square on the photograph, is distorted into a lozenge of peculiar shape, varying according to each observer's predilections, and it is very hard to identify the particular spot. So far as I could see on Dr. Rutherfurd's photograph there were two bright spots, each with a long dark shadow on one side, either of which would agree fairly well with Dr. Klein's crater. Dr. Klein in his diagram does not draw the crater any thing like so large as Mr. Neison represents. After all, there is a great deal of uncertainty as to the drawing of this part of the Moon.

Mr. Bidder. Whilst the discussion has been proceeding I have asked the Secretary to get me Schmidt's new map, and it confirms what Mr. Christie has said. If Mr. Neison correctly represents this part of the Moon, the map certainly does not. If you place them by the side of each other you will see that there must be a good deal of uncertainty about it.

Capt. Noble. When the announcement of Dr. Klein's discovery was made I examined this region of Hyginus, but failed to detect any thing; subsequently I received a communication from Mr. Birt, who said that there were so many discrepancies in the drawings which had been exhibited, that he asked me to look at the Moon and make a drawing myself, which I did on the 2nd of November, and I am bound to say that I entirely failed to see Klein's crater. One of the points to which Mr. Birt invited my attention was the discrepancy in the angle between the two rills in three separate drawings, one of Schmidt's, and I forget whose the two others were; and I found on running my micrometer over them that the angle subtended at Hyginus itself was 143° 30′. I asked Mr. Ward of Belfast if he would look at this region in the Moon; and he wrote to me that he saw Klein's depression only as a grey spot, and its position as determined by the micrometer with reference to Hyginus was: position-angle about 330°, distance 21″ from the crater. I have often looked at that region when it was pretty far advanced in sunlight, but I have failed to see the crater. I have not looked since I have had Mr. Ward's measures.

The President. I think the Society will be very much obliged to Mr. Neison for having brought this matter forward; but I do not feel so utterly smashed as I feared by Mr. Neison's criticisms, for he has rather carried out my point, that great care should be taken by everybody who thinks that he sees a new feature in the Moon. The only reason why I thought it desirable to bring forward my paper, which had really no argumentative character in it, was to bring to the notice of the Society the great changes, involving very large areas over the Moon's surface, which take place in a very short space of time. But I think Mr. Neison is rather in error in saying that the part where he placed the new crater was the part to which I wished to draw attention, which was really another part altogether, and had nothing whatever to do with the new crater. In the space of 25 minutes a perfectly bright white area became completely dark with a single bright spot by the side of it.

Mr. Gill. This seems to be a very troublesome crater indeed. But I should like to ask Mr. Neison, who is a promoter of the Selenographical Society, possessing an intimate knowledge of the neighbourhood of Hyginus, and knowing all about the effect of changes in illumination and so on, and who must have been excited by marvellous curiosity as to those changes in the Moon, why he has not, instead of coming before us and telling us what other people have seen, told us *what he himself has seen.* He has evidently not seen this crater, but he must have looked for it under most favourable circumstances. I should like to ask him why he has not brought some positive evidence, knowing as he does, the whole of the conditions which affect the visibility of the crater.

Mr. Neison. Let me take Mr. Gill's question first. I have endeavoured to observe this region at every opportunity since last April, but the weather has been so persistently cloudy that I have been unsuccessful. Capt. Noble remarks that Mr. Ward's measure places the crater in position-angle 330° and distance 21″. But he has forgotten the dimensions of Hyginus, and the effect of the inclination of the lunar axis; and allowing for these, Mr. Ward's measure brings the crater within 1-8th of an inch of the place I have given it on the drawing. Dr. De La Rue remarks that the crater is seen on the photograph as a white ring surrounding a dark centre. Dr. Klein says this is not the crater, and that the crater is never visible as a white object.

Dr. De La Rue. I did not assert its existence on the photograph. I merely asked the question.

Mr. Neison. There is an object on the photograph which looks something like what the crater might be imagined to be, though Dr. Klein says it is not in the right place; but the point is that all selenographers are unanimous that they did not see a great black crater here at sunrise, and now it is stated that there is one to be seen. Dr. De La Rue remarks that to some eyes it might be more conspicuous than to others.

Dr. De La Rue. I beg pardon: the point which I wanted particularly to bring before the Society was this, that an object barely visible to the eye may be very distinct in a photograph.

Mr. Neison. My point is this. It was not visible to the eye before 1877; it is visible now. There must have been some change which has now rendered visible to the eye what was not visible before. Whether a photograph shows a marking here or not from this point becomes immaterial. Mr. Bidder draws attention to the fact that my drawing differs much from Schmidt's map. This is true; but it must be remembered that Schmidt never intended his map to be reproduced in the way it was, and it shows tiny ridges as conspicuous as big mountains. With regard to Mr. Christie's remarks about the discrepancies in the different drawings between the positions of the small craters, I would remark that it is very difficult to accurately delineate the position of these in very small detail. They are very small, though the white spots in their position on the photographs are larger. I may add that I mentioned Lord Lindsay's name in connexion with the new crater on the basis of his having remarked that he looked for the new crater and did not see it. With regard to his observation of an apparent change in a formation south of Hyginus, I think, as I before remarked, that is nothing more than what every observer of the Moon is thoroughly accustomed to; objects do seem to change in a striking manner before sunset.

The President. I did not say anything about a change of shape or position. I merely said that what was nothing at all before—because there was nothing to be seen—suddenly became dark.

Mr. Neison. Certainly. Until the sun has almost set nothing is visible, for the slopes of the object are so gentle they cast no shadows; but just before sunset the entire slope is swiftly thrown into black shadow, and a mere dimple or slight depression takes the form of a very black object. It is not Lord Lindsay's paper that I have replied to, so much as the reports of his paper, which do seem to convey an erroneous impression. The idea was conveyed that the region around Hyginus had certain peculiarities which ought to render us suspicious as to the reality of apparent changes in it, whereas Lord Lindsay's paper makes out no such circumstance. Moreover, by cautioning observers not to take a crater as new simply because it had not been noted before, you do seem to convey the idea that this is done. Is it?

Dr. Wentworth Erck. Something was said as to the difficulty of getting a bichromate battery which should always be ready for use at a moment's notice; and having contrived one myself, I thought I would like to show it to the Society. The internal resistance is less than that of Grove's cell; the current is constant; you can increase the resistance as you please, and you can easily bring it down; you have it under complete control; it is always ready at a moment's notice, and takes no time to put in operation. The space between the zinc and the carbon is just sufficient to prevent accidental contact between them, and it will allow the bichromate solution to flow constantly, so that the zinc is in the position of a ship anchored in a canal with a constant current on each side of it. The peculiarity is the extraordinary extent to which you may dilute the bichromate solution and yet get a strong current. Here is zinc which has been in operation for six months, and you only have to drop it in this manner, and it is as good and as convenient for use as a common bichromate battery, and it is available at any moment.

Mr. Gill. The great fault of all bichromate batteries is polarization. Does this prevent that by the actual current flowing through the battery? and if so, is it troublesome to keep it up?

Dr. Erck. There is a constant current at the rate of 6 oz. per hour, and with that flow of current the polarization is practically nothing.

Dr. De La Rue. What steps did you take to ascertain the constancy of the current?

Dr. Erck. By attaching a wire to a galvanometer, and the deflection remained constant.

Dr. De La Rue. If the flow was stopped, did it remain constant?

Dr. Erck. No.

Mr. Ranyard. Then a certain change in the rate of flow makes all the difference in the strength of the current.

Dr. Erck. The strength of the current varied with the strength of the solution and with the depth of the zinc. It is a very convenient thing to start with the ordinary solution diluted one half. I use it with a piece of zinc about ¼ in. wide, and when the solution has passed several times through the cells I use a larger piece of zinc. According as the solution gets strength I put a little more zinc.

Mr. Ladd. My experience is that such batteries are very far from being constant. I find that those cells which are the most constant are those that hold a great deal of solution, and where there is a considerable depth below the carbon or the zinc plate, in order that the bichromate, when it becomes decomposed, may sink to the bottom. If I were going to make a new bichromate cell I should not adopt the plan proposed by Dr. Erck, because there seems to be no room for the decomposed solution to go to the bottom.

The President. My own experience of a bichromate battery is very much what Mr. Ladd has mentioned, and I have given up using it. But if the fluid is constantly changed, as is done in some laboratories in Germany, by having the battery on a shelf and a large bottle above dripping into it, that is a very material improvement. At the same time I agree with Mr. Ladd as to the advisability of having a considerable depth of fluid below the battery, so that the decomposed chemicals might fall by their own gravity, and no longer offer such resistance.

Dr. Erck. Mr. Ladd's experience as to batteries, no doubt, is what he has stated, but he has had no experience of such a battery as this; and I submit that the experience of other batteries has nothing to do with this battery. With regard to having a great quantity of solution below the plates I thought of that myself, and in some experiments that I was trying I had as great a depth as 14 inches below the plates; but I found that it would not do, for the reason that the products very easily sink to the bottom, but that sinking to the bottom is counteracted by heat. You have a constant stirring up of the fluid from top to bottom. This is not one of those cases like the craters of which we have heard so much: I am prepared to show any gentleman that with that particular apparatus I will maintain a constant current as long as that amount of solution in the bottle lasts. As to Lord Lindsay's remark, that a similar battery has been found inconstant, in mine there is a constant current without any stop whatever, unless there is very great mismanagement.

The President. I said if you had a bottle above and a stock of fluid. The change of fluid is not a new idea.

Dr. Erck. No.

The President. May I ask what force one gets from a battery of this description, and under what circumstances one could keep a closed circuit, and for how long, or a circuit which would do for the work of an electro-magnet, which circuit has to be closed for a considerable period of time?

Dr. Erck. For the particular purpose of closing the circuit in an electro-magnet, for something under half a second, the wire round the magnet would be No. 16. We get a current which will remain perfectly constant, so long as the solution is there, at the rate of 6 oz. an hour.

Mr. Ranyard read a paper by Prof. Asaph Hall on Hyperion, in which it was pointed out that owing to the attraction of the mass of Saturn's ring and of Titan, the line of apses had advanced 180° since 1848.

Mr. Plummer. I wish to make a remark regarding a discussion which took place at the last meeting, and which was brought on by Mr. Christie, with reference to abnormal phenomena which sometimes take place at the occultation of a star by the Moon. About 50 years ago the late Sir James South read a paper before this Society, in which he carefully examined all the cases of the projection of stars upon the Moon's limb which he could possibly collect; and he further examined the five hypotheses which were then current as to the explanation; and any one carefully reading that paper will admit that he succeeded most effectually in exploding them all; and yet in spite of that whenever an abnormal phenomenon of this class takes place some one or other of those exploded hypotheses is brought forward to explain what has been seen. I think on the last occasion one gentleman referred to the colour of the star: that is one hypothesis which Sir James South investigated, and which he had to throw aside as utterly hopeless. Since that time two other hypotheses have been started with the view of explaining these phenomena. The first was referred to by Mr. Gill on the last occasion, viz. that if a star disappeared at a portion of the Moon's limb where there was a deep valley that valley might contain some remnant of a lunar atmosphere, and in that way the star by refraction through that atmosphere be projected on the limb. Now observations of this character have been verified by observers in very different parts of Europe on the same occasions; and as parallax would shift the position of the star with reference to the Moon, it would be extremely strange if a valley of this character should always happen to be in that position for every observation. The explanation suggested by Sir George Airy can hardly be described as an hypothesis, because it is founded upon certain optical facts which we know do take place; and therefore we must conclude that at every occultation of a star by the Moon, the phenomenon which is described by Mr. Christie must actually take place whether it is perceived or not. If Mr. Christie is satisfied that this explains all that he saw, I have no objection to accepting his explanation; but I wish to point out that it will by no means serve as an explanation of all the cases of the projection of stars upon the Moon's limb. No one who has actually observed the projection of a bright and conspicuous star upon the dark limb of the Moon, visible by means of earth light, will ever be satisfied with that explanation. It is not merely that half the disk of the star and half the rings are projected upon the lunar disk, but the whole of the rings are not only within the disk, but well within the disk. The 'Proceedings' of this Society give instances in which observers have stated that a star has been projected to the extent of one minute, and even two minutes, of arc within the disk, though I think in this there must have been some unconscious exaggeration. From my observation, which was taken ten years ago, there was a projection of not less than 7″ and probably not more than 10″. No doubt there are a large number of instances where the projection has been well within the limb to an extent far exceeding all that could be accounted for by an optical increase of the Moon's limb according to the explanation of the Astronomer Royal. Some years ago, having given attention to this subject, I suggested another hypothesis which I am sorry to say has not, on the whole, approved itself to the attention of astronomers. My suggestion was that a lunar atmosphere in connexion with the libration would explain the phenomenon. If it can be shown that no marked case of projection occurs except where there is a considerable amount of libration, we shall have established one fact to go upon. I have examined all the cases that I can find between the years 1850 and 1870; they are 13 in number, and, omitting two as doubtful cases, there are 11 which coincide in giving a distinct positive libration at the point where the star was seen to disappear; but to be perfectly ingenuous I must say that there was one observation taken at Wilna which by no means agreed with my hypothesis, and for that reason I have for some years said nothing about it. The best way to test my theory would be to make a number of observations all round the Moon's limb (which might be possible at the occultation of the Pleiades), to afford a chance of observing at the same time the different behaviour of the stars at different points on the Moon's periphery. I am able to confirm to some extent Mr. Christie's observation of the star 17 Tauri, which was certainly clinging or attached to the limb for the space of ½ a second, according to my observation; and I found that there was a slight positive libration at that point; so far as that goes it is favourable to the hypothesis. Another star seemed to cling a little more, but the observation was of such a character as to afford only negative evidence on the whole. We shall have the Pleiades occulted on another occasion, and it will be an interesting point for astronomers to watch carefully which of the stars appear to cling more than others.

Mr. Christie. May I ask how an atmosphere will account for the projection on the Moon's disk?

Mr. Plummer. There can be no question, I think, that when the ray from a star is grazing the surface of the Moon, and afterwards passing through a certain amount of atmosphere, it must undergo precisely the same refraction as the limb itself, and therefore in every case it seems to me that the star must really disappear at the limb. I must therefore improve upon the hypothesis; and I do it in this way, which I trust will give satisfaction, to some minds at least. The phenomenon appears to me something analogous to mirage. If there is a portion of the Moon's surface highly heated and covered with an atmosphere, the portion which is quite near the limb will be rarefied by the extra amount of heat, and it may be somewhat denser at an elevation from the body of the Moon. If that be the case the ray of a star passing through that denser atmosphere will be deflected and the object will appear upon the disk. It must suddenly come back, but it may be so sudden that the eye is perfectly incapable of perceiving it, for I do not imagine that the stratum of heated atmosphere can be of any considerable depth; and I do not think it necessary to suppose that the atmosphere is of a density comparable to our own.

The President. Supposing that there was an actually increasing refraction, as it approached the limb of the Moon, would not the star appear to go very much faster?

Mr. Plummer. Practically it does. I have only seen the phenomenon once, and everybody knows that it is an extremely rare thing. The circumstances under which I saw it were distinctly those of which Lord Lindsay has spoken. I was observing the occultation of a star in Taurus, not expecting any thing of the kind. The star approached as usual up to the limb, and I waited expecting an instantaneous occultation. I cannot tell how long, but in an instant it was some distance within the limb, and it remained there for the space of five or six seconds according to my belief, long enough to make quite certain of the fact. I do not profess to say that I saw the star go across the limb; but I saw it at the limb and I expected an instantaneous occultation, and then it suddenly appeared considerably within.

Mr. Dunkin. The observation of Mr. Plummer's is really a very important one, if he has actually seen the star on the dark limb. I can hardly understand how it is possible; but as a practical matter I may say that I have observed many occultations both at the dark limb and at the bright limb. I have seen the projection of Regulus on the bright limb, and have seen it distinctly between two or three seconds inside the limb of the Moon; but during my whole experience I have never observed any thing of the kind on the dark limb.

Mr. Plummer. The observation of a star visible within the dark limb is by no means unique. There are many instances mentioned by Sir James South even 50 years ago.

Mr. Ranyard. I cannot say I understand Mr. Plummer's theory with regard to the reflection of light from the surface of a denser stratum of atmosphere; but I think it may be taken as a full answer to the theory that the brightness of a reflected image depends upon the difference of the refractive indices of the two media at the surface of which the reflection takes place, and mirage effects would consequently be very faint within the lunar atmosphere.

Mr. Christie. As to the projection of a star on the bright limb of the Moon there is no difficulty in explaining that by the theory which the Astronomer Royal has put forward. It must not be understood that in that case the irradiation is confined to the spurious disk of the star; but when you have an overpowering light from the Moon you may have an irradiation of 2″ or 3″ very easily. As Mr. Ranyard has pointed out, there is great difficulty in accepting Mr. Plummer's theory in the case of the bright limb; and I fail to see how it can possibly explain any projection on the dark limb of the Moon, because there can be no heat from the Sun whatever in that case so far as I see. Considering that the lunar atmosphere is very rare, and we have Lord Rosse's observation that the Moon's temperature falls very rapidly, I do not think that the theory could fairly explain any abnormal heating of the lunar atmosphere.

Capt. Noble. I cannot help thinking, in the first place, that the excellence of the telescope with which an occultation of a star is observed, and, in the next place, personal equation, must have a good deal to do with these abnormal phenomena. I have observed many hundreds of occultations with a telescope of great excellence, but I never had the luck to see any thing in the shape of a projection on the

Moon. I have seen the Moon go over a star very obliquely, and I suppose the Moon's edge was serrated, for I saw the star come out and disappear again; but whether that would give rise to any thing assimilating to a projection on the disk I am not prepared to say.

Mr. Gill. I did not advance the hypothesis of an atmosphere; I merely asked, as the star approached a hollow, had the old bugbear of a lunar atmosphere any thing to do with it?

Mr. Plummer. I think I must have misled Mr. Ranyard. I did not speak of reflection. I said the phenomenon was like the mirage. The heating of the surface produced a difference of refraction. I can only recommend Capt. Noble to read Sir James South's paper. He has started two of the hypotheses which Sir James South has shown to be fallacious.

Mr. Downing read a paper on the Reduction of the North Polar Distances of the First Melbourne General Catalogue to Auwers's Standard. This investigation consists essentially of a comparison of the North Polar Distances of the Cape Catalogue for 1860 with those of the Melbourne Catalogue; the reduction of the latter to the Standard is then accomplished by applying the corrections to the N.P.D.s of the Cape Catalogue given in a paper read before the Society at the December Meeting. There are 352 stars available for the comparison.

Proceeding exactly as in the comparison of the Greenwich and Cape Catalogues given in the paper referred to above, a curve has been drawn representing the systematic differences between the catalogues. The differences, Cape *minus* Melbourne, as read off from the curve, begin at $-1''\cdot2$ at N.P.D. 50°; the curve then rises rapidly towards the axis, cutting it at about N.P.D. 65°: it continues to rise gradually in the positive direction to about N.P.D. 145°, where the difference is $+0''\cdot8$; the curve then turns back towards the axis, cutting it near the South Pole. The reductions to Auwers's Standard range from $-0''\cdot9$ at N.P.D. 50° to $+0''\cdot44$ at N.P.D. 100°; and the reductions to Henderson (for stars south of N.P.D. 120°) range from $+1''\cdot7$ at N.P.D. 120° to $-0''\cdot24$ at N.P.D. 172°. This latter part of the investigation is of course provisional only; it is to be hoped that when Mr. Stone's great catalogue is published it will be possible to deduce a system of Standard North Polar Distances for southern stars analogous to that given by Dr. Auwers for stars visible in European latitudes.

Mr. Gill. I wish to remark with great emphasis upon the very important work which Mr. Downing is doing. No one can read his last paper without seeing the great amount of labour, care, and enthusiasm for such subjects which dry figures do not engender in many people. But it is only those who are engaged in discussing the question of absolute declination who can appreciate the value of such a paper as Mr. Downing's; and it bears with very great interest upon a matter of the highest importance in practical astronomy, viz. the question of astronomical refractions. In the last annual Greenwich catalogues a correction has been applied to Bessel's refractions. The tendency of these discussions is to induce a stronger belief in the goodness or the truth of the old refractions without this correction.

Mr. Christie. I agree with all Mr. Gill's remarks as to the importance of Mr. Downing's work with regard to this question of refraction. As regards the Greenwich catalogues, the old refractions were used up to 1868, and the new refractions (that is, Bessel's refractions diminished in a certain ratio) from that time in the Nine-Year catalogue, which is now about to be published. I admit there are some differences as compared with previous catalogues. At any rate the matter of astronomical refraction is one which can only be satisfactorily settled by referring to southern observatories, because there are peculiar difficulties about observations of circumpolar stars, which make it very difficult to deduce the refraction with absolute certainty from them, so that Mr. Downing's discussion in this respect is very valuable. Both of his papers show that the old refractions sensibly represent the observations made in southern observatories.

The following papers were announced:—

A. E. Nevins. On the practical advantages of Hartnup's method of testing Chronometers.

Prof. R. Grant. Observations of the Transit of Mercury, May 6, 1878, and of the Occultation of Mars by the Moon, June 3, 1878, made at the Glasgow Observatory.

B. G. Jenkins. On a possible connexion between the movements of the Magnetic Needle and the planet Neptune.

A. Marth. Ephemeris for determining the Positions of the Satellites of Uranus, 1879.

Lord Lindsay and Dr. Copeland. Note on Meteors observed November 27, 1878.

The following were elected Associates.—

Prof. Asaph Hall; Prof. A. Krueger; Prof. C. H. F. Peters; Prof. H. C. F. C. Schjellerup.

The following were duly elected Fellows:—

Lieut. C. W. Baillie, R.N.; Fredk. Wm. Clerke; Thos. Cushing; Robert T. A. Innes; Rev. J. Holdsworth Morgan; Charles Pendlebury, B.A.; Prof. C. W. Pritchett, M.A.; Capt. James Rankin; Dr. Arthur W. Wright.

The meeting adjourned shortly after 10 o'clock.

The Meteors of Nov. 23–30, 1878.

It was reasonably expected by astronomers that there would be a repetition this year of the remarkable meteor-shower recorded Nov. 27, 1872, and known to be fragments of Biela's Comet; and no doubt close watch was kept by meteor observers during the latter part of November for the expected display; but, as far as the observations of the writer show, very little, if any, trace of the shower was seen. For a week previous to Nov. 23 (with the exception of a short watch obtained on the 21st) the sky remained generally overcast, and a repetition of this unfavourable weather prevented observations on Nov. 25–27 and 28, and on Dec. 1, 2, 3, 4, and 5. The moonlight also proved exceedingly troublesome after Nov. 28 and prevented the recording of any but very bright meteors.

A careful watch of the sky (ranging from one to four hours in duration each evening, and aggregating some 17 hours in all) was kept on Nov. 23, 24, 26, 29, and 30, and Dec. 6, 7, and 8, for any meteors conformable to a radiant near ν Andromedæ; and during this time only 3 meteors were mapped that belonged with any certainty to the Bielas—one being recorded on the 23rd during a watch of three hours, and two on the 26th during a four hours' watch; and the paths of these prolonged backwards met exactly at R.A. 25° +42°, and they were undoubtedly members of this stream. Being interested to know whether the shower had been seen at other stations in the U.S., particularly on the 27th, I wrote to Profs. H. A. Newton, D. Kirkwood, and Cleveland Abbe, and received from these gentlemen negative results as far as they were able to find out; so that it is reasonably inferred from this that no considerable part of the Biela stream encountered the earth at the nodal passage this year. Although the Biela shower was conspicuous for its almost entire absence, still the following radiant-positions were well deduced from the meteors mapped, some 50 in number.

1.	Nov.	24–29,	R.A.	25°,	Dec.	+26°,	5 meteors.
2.	„	23–29,	„	76°,	„	+46°,	8 „
3.	„	26–29,	„	81°,	„	+21°,	9 „
4.	„	24,	„	330°,	„	+63°,	4 „

1. Meteors quite bright, short, and slow, strongly suspected. Denning, from Italian Met. Assoc. Obs. 1872, at R.A. 24° +27°, Nov. 25 to Dec. 31.

2. Position exact. Meteors rapid, short, and generally faint. Three recorded in rapid succession on the 24th. Confirms a radiant deduced at Pola, Nov. 27, 1872.

3. Taurids II. Meteors generally bright, short, and slow. Radiant deduced last year at R.A. 80° +22°. Principally seen on the 29th.

4. Strongly suspected. Meteors faint, short, and rapid. All seen during one hour's watch in the W. Denning from Italian Met. Assoc. Obs. 1872, Nov. 25–Dec. 31, at R.A. 330° +66°.

Cambridge, Mass., U.S.A., Edwin F. Sawyer.
1878, Dec. 13.

Captain Abney's Book on Emulsion Processes[*].

At the present time it is almost necessary that every astronomer should be a photographer. The "optic tube" of past times has for the moment become somewhat unfashionable; seeing is no longer believing; and the astronomical phenomena that are engrossing most attention are those that you cannot see with your mortal eyes[†].

This phase of our experience will doubtless soon pass away; but it is none the less necessary for us to live in our own times, and, in order to do so, the sooner we acquaint ourselves with photography the better. We have been obliged to learn such things as the undulatory theory and the molecular theory in connexion with modern astronomy, and now we are obliged to learn the practice of photography; and though at first they may all appear sufficiently repulsive subjects, some few steps of knowledge are apt to enchant them into tolerable roadways if not pleasant fields, whilst their skilful use may even convert us into enthusiasts.

To speak briefly, the book I have to review is a book upon dry plates; how to make them according to diverse authorities, and how they are to be used, together with a short discussion of their theory. It forms a sequel to the author's 'Instruction in Photography,' which is a practical treatise on the whole subject.

Dry-plate photography is at present by far the most interesting and useful form of the art, and the simplest way of preparing the plates is by an emulsion process.

An emulsion process implies not merely a dry plate, but one in which the silver solution is incorporated with the collodion in a bottle, instead of the collodion film being spread first and then dipped into a bath of silver solution, and may be considered generally as an improvement on the older method.

There are two other leading peculiarities of emulsion processes: one is that gelatine may be substituted for collodion with advantage for holding the sensitive salt; the other that the salt employed may with advantage be the silver bromide alone, instead of the iodide and bromide combined, and by these means a very short exposure suffices for obtaining an image.

The quickest processes now in use are on films of gelatine impregnated with silver bromide and developed with ferrous oxalate; and under favourable conditions they can be worked with about a third of the exposure required for a wet plate. It is hardly necessary to remark that the practice is subject to risks in proportion to its rapidity, so that more dexterity and experience are required than with the slower methods. It is obvious especially that over- or under-exposure in the camera are more fatal when the admissible latitude is so very small. And on this point I will make a criticism parenthetically:—nothing is more puzzling to a beginner than to decide whether or not his negative was sufficiently exposed. Such density of image can be got with the alkaline development, that he at first refuses to believe in its under-exposure; and it would help him much in his troubles if the effects of this mistake were more fully and clearly described.

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[*] Photographic Handy-books.—No. 2. Emulsion Processes in Photography. By Captain W. de W. Abney, R.E., F.R.S. London: Piper and Carter, 1878.

[†] Two at least of our most distinguished and most industrious observers have not looked through their telescopes for several years, having converted them into photographic instruments.

The book commences with a general account of the process, describing the washed and the unwashed emulsions; then collodion is dealt with in detail, and the author goes on to a theoretical dissertation on sensitive films, on alkaline development, and very particularly on the causes and elimination of that bane of photography known technically as "fog."

I cannot say that this part of it is as clear and consecutive as it might be; at all events it is not by any means brilliant; and from Captain Abney we expect no less. I think it would be quite worth while to rewrite it, so as to unfold the ideas in more orderly sequence, for as they stand it is no great exaggeration to say they are shot down like peas in a bushel.

Notwithstanding this disadvantage, the information given as to the causes and the elimination of "fog" is probably the most valuable part of the book; but after the author has done his best to clear up these difficulties with, as I think, a considerable degree of success, he still has lurking misgivings in his own mind, and gravely invites his reader to a most attractive enterprise in the following words (p. 10):—"If any reader will not admit it, he should read all the various evidence that has been adduced since the time when Scheele first made his experiments with silver chloride; and we doubt if he can remain unconvinced" !!!!

On this the sceptical reader is supposed to retire hastily into his shell.

It is not easy for an ordinary student to get acquainted with the literature of photography, for the simple reason that all that is original in it is scattered through multitudes of papers in various periodical publications which are practically inaccessible to him. It is not likely that such lively reading as the photographic newspapers would engross any one's time who was not specially devoted to the art and an enthusiast in it too. The truth, however, is that all scientific photographers *are* enthusiasts, and most of them are amateurs, the one to whom we owe most as a systematic writer on the subject being by profession a soldier *.

This alone is sufficient to corroborate the assertion I will flatly make, that a more fascinating pursuit than photography does not exist. It presents an instance of science inevitably mixed up with art; and art not merely in the sense of skilful manipulation, but an artistic sensibility to beautiful results. Nothing, for instance, can be more purely scientific than Captain Abney's account of the change of the bromide into the sub-bromide by the action of light, and the limitation of the activity during development; nothing more artistic than his appreciation of the melodies of shade that ensue (he calls them harmonies).

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* It is deeply to be regretted that the R. A. S. should just now lose the Fellowship of this able investigator. It will henceforward be understood that the Society is gifted with singular taste, and whilst it chooses to keep on its Council a person whose only distinction is an unauthorized military title, it can very well dispense with officers in the Queen's service, however great their scientific attainments.

Although it would be flattery to call this a well-written book, there is yet a tone about it that is peculiarly attractive. It betrays in every page a most serious endeavour to make a rough road smooth, and to rake out into the light all the obscurities and subtleties that characterize a complicated subject. You feel immediately you begin to read that you are in safe hands. There is an undecorated truthfulness in every line which secures your adhesion; and even in passages that you cannot understand without frequent re-reading you persevere willingly, feeling that no less is due to a teacher who is bent on helping you. It will readily be admitted that the attitude of the docile pupil is a most pleasant one to assume, and a reliable tone on the part of the teacher immediately secures it; but I suppose we have all of us at some time been driven to the conviction that it is not from the accomplished performer that the most effective help can usually be obtained by the beginner. The master may too often be likened to a man who in his early struggles "beheld his natural face in a glass," and, once having got through them, "straightway forgetteth what manner of man he was;" so that if he be not endowed with imagination or a memory which is its twin brother, the state of helplessness whence he long ago emerged has passed so completely from his mind that he is liable to fail signally in conceiving of a beginner's difficulties in any thing like their true relative magnitude or the true order of their occurrence in his experience.

For this reason I think it not necessarily immodest for the pupil to rise up in judgment against his teacher, as I am doing at the present moment.

In Captain Abney's book the information is so thickly packed and is stated with so little emphasis that after reading it through with even a decent degree of attention you will set to work with confidence and make nearly every blunder which could possibly be made; and then in the evening you sit down hopefully to see whether the "handy-book" says any thing about your last and most hopeless mess (it is essentially *unhandy* in not having the contents of each page printed at the top), and to your delight you find the very case set forth fully. You try again next day and again fail, and, very sick, you set out to wade through the book again from the beginning; and the fact dawns upon you that you had been reading so carelessly as to miss the most essential points. Now of course a good pupil ought not to do that, and it may be said that he has only himself to blame; but I am not willing to admit so much: I consider it a fair reflection upon the book—not certainly on its information, but upon its writing. Man is mortal and cannot be expected to sort out and set in order a parcel of ideas with which he had no previous acquaintance. This is why good writing is such a priceless gift: it is the cooking of intellectual food so as to make it first palatable, then digestible by weak and distracted mortals; and it is matter of continual amazement to me that science students of eminence, who have been trained in all that demands order and accuracy, should yet be so insensible to measure and accent as to stand up, according to their custom, before the so-called learned societies and gabble or mumble their speech in such fashion that half of it cannot be heard and the other half cannot be understood by any living creature; and then the President gets up and invites a discussion; and a dead silence or a shot quite beside the mark is the only result. And truly the secretaries of our societies are amongst the worst offenders; and any man may excusably say that it is useless to write a paper well when it is to be read off, as in one society, by a steam-shuttle, or in another by a mouse squeaking through a bag of wool.

I occasionally listen to one scientific secretary, who when about to squeak executes a little apologetic dance like the fluttering of a small and lame bird. These are the examples that the Fellows have always before their eyes, to our humiliation be it said. I wonder whether the introduction of art at the universities will make the speech of the next generation more melodious! Possibly the making of Latin verses has been mistakenly neglected by the modern boy.

There is another trivial remark to be made here. It is not decent to put beautiful workmanship into miserable material, and we ought not to expect a valuable book like this to be sold for half a crown. A book from which we may learn an art adapted to extend indefinitely our acquaintance with the universe, and to make our lives altogether more delightful, ought to be a dainty object to keep, not a thing done up in a piece of yellow paper. A small cheesemonger in Whitechapel might conceivably present his goods in that way. Printer's errors also are needlessly frequent even for a cheap book; but there is another defect that I ought to point out, which runs through the whole of both parts, viz. an inconsistency in the supposed previous knowledge of the reader.

For instance, one so ignorant of chemistry as to require to be told the atomic weights of the commonest elements, and to have every equation written out for him in words, would not be likely to know what is meant by a "halogen." A man need not be a dunce because he has no conception what is a haloid and what is not. It is no doubt always a difficult question how much previous knowledge to suppose on the part of your reader, but the standard assumed ought to be kept in mind consistently throughout.

The author's "Text-book," published by Longman, is better in all ways, and should be read by every student; but to the uninstructed reader the grammes and centilitres that are thickly scattered over its pages reduce (for all practical purposes) a delightfully lucid book to a senseless mass of paper and ink. If he doggedly declines to "learn his tables" afresh he must content himself with the "Handy-books;" and thankful he should be too. The "Text-book," however, contains an interesting outline of the history of the art which is well worth reading for its own sake. Photography is still young and behaves like a gaunt spectre with small regard for his votaries. There is no popular consideration or honour attaching to his service. He goes stalking on over the whitening bones of philosophers upon whose lives he has fed, and who have sacrificed to him all but their dried names, which we are beginning to look for in biographical dictionaries. Of them it may truly be said, when they retire into the unseen, that "their works do follow them."

We have seen photography in his infancy in our own time: this will be a great age to have lived in if we should see also his maturity.

There is reason to expect that some of us may do so. He who has actually photographed the red end of the spectrum may conceivably go further and fix natural images in their true colours. Then, I think, we shall have lived to see the achievement of the greatest enterprise yet undertaken in science. It has been affirmed that photography is the straight gate that leads to a scientific education. Be this as it may, I believe that as a moral training it is unsurpassed. Patience under adversity is always hard to learn; but photography is a master with a winning voice, and whilst learning the art you will also learn to use moderate forms of speech. Its kingdom is not one that "suffereth violence."

JOHN BRETT.

Double Stars for February.

THE principal objects from 4^h to 6^h have already been given. The following may also be carefully measured. The existing measures of one of them, Σ 554, are very discordant.

Σ 3114. R.A $4^h\ 1^m$, Dec. +39° 51′. Mag. 8, 10·5.

1864·64 179°·4 2″·26 Dembowski.

Binary. Not measured since 1864 probably.

Σ 511. R.A. $4^h\ 7^m{\cdot}8$, Dec. +58° 29′. Mag. 7·5, 8.

1870·25 294°·3 0″·6 OΣ.

Binary. Difficult.

Σ 531. R.A. 4^{h} 14^{m}·1, Dec. +55° 22′. Mag. 7·4, 8·6.

1871·26 295°·2 1″·07 OΣ.

Probably binary. Measures wanted.

Σ 535. R.A. 4^{h} 16^{m}·8, Dec. +11° 5′. Mag. 6·7, 8·2.

1877·16 335°·6 1″·72 Spörer.

Binary in rapid motion. In 1829 Σ found the following:—355°·0, 1″·96.

Σ 554. R.A. 4^{h} 23^{m}·3, Dec. +15° 23′. Mag. 6·5, 9.

1874·07 14°·7 1″·29 Wilson.

A physical system. Difficult to measure well. Common proper motion in R.A. +0″·061, and −0″·003 in N.P.D.

JOSEPH GLEDHILL.

Meteor Notes for February.

OBSERVATIONS A.M. may be made during the last half of the month, and P.M. in the interval between the 10th and 25th. No special showers are due this month; but there are a few active radiants at 158° +29°, 180° +56°, 136° +69°, 75° +44°, and several of the positions given for last month are continued in this. There is also a radiant in Orion seen by Schmidt at 80° +6° on Feb. 28; and Zezioli recorded (evening of Feb. 24, 1868), a "numerous movement of bright falling stars, most of them directed from the three stars in the girdle of Orion towards Rigel." He enumerated 23. On Feb. 15, 1869, 8.15 P.M., he also notes "many stars departed from the semicircle of Ursa Major around 135° +58°;" and on Feb. 16, 1868, 16–16½^{h}, he observed a "movement of swift stars with long paths from the point 214° +54°, but uncertain, owing to the brightness of the Moon." In singular conformity with this, he saw (A.M. Feb. 16, 1869) "meteors in Canes Venatici and Boötes which seemed derived from 214° +53°, but clouds impeded each determination." This refers to the same stream as that deduced by Schiaparelli at 209° +52°, Feb. 14–15 (see radiants nos. 35 and 38, S. and Z.)*. A shower at 208° +17° (near η Boötes), seen by several observers, should also be looked for this month and, at the end, a system of slow meteors with a centre close to β Trianguli (30° +35°), which offers a good accordance with the comet of 1764 (Feb. 25, 33° +33°). This cometary radiant is well supported by a stationary meteor observed by Schulhof at Vienna at 33°·8 +36°·5 on the evening of Feb. 23, 1870. Several radiants near the equator, slightly seen by Tupman in 1869, Feb. 14, A.M., at 202° −9°, 205° +4°, 237° +13°, 230° −7°, and 260° ±0°, may now be re-observed.

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* Zezioli was a very successful keen-sighted observer; and one is struck, on looking through his Catalogue, at the large number of meteors he often saw during his long watches.

Recent Observations.—Mr. Edwin F. Sawyer publishes ('Science Observer,' Nov. 1878) a list of 49 bright meteors (equal to or exceeding 1st-mag. stars) recorded by him at Cambridge, Mass., during the three months July 22–Oct. 29, 1878. Further, similar lists are promised, and the accumulation of such materials—the result of much assiduous observation—cannot fail to prove of considerable value.

On December 21 many slow and several brilliant meteors were observed by the writer at Bristol. At 9.33 one > ♀ fell from 13° −5° to 11° −19°, and left a short streak of not more than 1° for about 7 sec. at the end of its path. Before maximum the nucleus became very faint, then suddenly increased to a vivid flash, and the streak remained just at the point of greatest brightness. At 10.9 a slow 2nd-mag. meteor with a bright train of sparks travelled through Perseus from the direction of Cassiopeia, and a moment later another > 1st mag. shot down through Orion from a point near β Tauri. Path from 79° +3° to 79° −13°; no train; medium speed. The latter belonged to a radiant at 79° +25°, which seemed the chief shower on Dec. 21–22 this year, and agrees with the *Taurids* II., though the date is late for them. On Dec. 21, 7.11 P.M., a meteor = ♂ was seen by the Rev. S. J. Johnson at Crediton, Devon. "It took a diagonal course from 20° below Mira Ceti almost to the horizon. Very low altitude. Direction from Taurus."

At Bristol, after a heavy fall of rain, sleet, and then snow on the night of Jan. 1, the clouds partly cleared away, and between 6.14 and 6.34 A.M., Jan. 2 (in a sky fully two-thirds overcast), 14 meteors, all of them belonging to the special shower in *Quadrans*, were observed. Evidently this radiant was very active at that time. The paths were short and quick, without streaks or trains. Radiant-point at about 230° +51°, but not exactly found owing to the clouds and haze through which several of the meteors were indistinctly seen. Three or four were as bright as 1st-mag. stars.

Dr. N. de Konkoly sends Part III. of the "Observations of Shooting-Stars in Hungary," chiefly at O-Gyalla and Zágráb, during 1877, July 26–Aug. 13. The list includes 240 paths, most of them recorded by Drs. Schrader and Weiss at the former observatory. On Aug. 10 Dr. Schrader determined a radiant at 32° +57°. On Aug. 11 the chief positions were at 44° +60° and 345° +17°. The first of these at χ Persei is one of several well-ascertained radiant-centres of the *Perseids*.

W. F. DENNING.

CORRESPONDENCE.

To the Editor of 'The Observatory.'

The Pulkowa Double-star Observations.

Sir,—

I have read with great satisfaction in your journal the full and very able account, drawn up by Mr. Downing, of my micrometrical measures, published in Vol. IX. of the Pulkowa Observations. There are, however, in the paper two suggestions, about which I ask your favour to insert a few remarks in the 'Observatory.'

Mr. Downing complains that I had not made use of the double-image principle, and that I had not employed for those measures the double-image micrometer devised by Sir G. Airy. I do not wish to enter here on a discussion of the relative merits of double-image or wire micrometers, and I willingly acknowledge that my admired friend the Astronomer Royal has, in a great measure, overcome the difficulties that formerly existed in the double-image micrometer, and that he has succeeded in making it an instrument well-qualified for measures of highest accuracy. What I want to point out here is merely the fact that Sir George's construction dates only from 1845, and that it was considerably later than 1850 that, on his suggestion, the double-image eyepiece was brought to its present state of perfection. At this later epoch the greater part of the measures published in Vol. IX. were already made. This statement, I expect, will be sufficient to prove that it was much more to my purpose to continue on the same method followed until then, and to inquire afterwards into the systematic errors of this method, than to go over to a quite different mode of observation, even if its superiority were less questionable than it is in the present instance.

Further, Mr. Downing advances the supposition that double-image measures would be much more free from personal equations than those made with the wire micrometer. I confess I do not conceive on what considerations this supposition is based; certainly it is a supposition that other astronomers would not accept without direct experiments. However perfect an apparatus may be in its principle, there is no doubt the personality of the observer will affect more or less the result to be obtained with it; and the more perfect the instrument is, the more the observer will feel obliged to deliver also the results from the influence of his own individuality. Consequently if Mr. Downing admits that the double-image micrometer was the superior instrument, he should also admit that the struggle with the personal equations inherent to its use would have had to be even harder than for the wire micrometer.

In this respect it is worth seeing what enormous quantity of labour Prof. Kaiser of Leiden—to whose observations Mr. Downing refers—has had to perform with Airy's double-image micrometer before he got a limited number of satisfactory observations for about 60 double stars. This number of observations is too small to give any law of the systematic errors, with which they might be still affected, but it is sufficient to show that at least the measured distances are not free from constant errors. Having compared 55 distances almost simultaneously measured by me, I find in the mean, for a distance of 4″·0,

$$K = O\Sigma - 0''\cdot079 \pm 0''\cdot018.$$

At the distance of 4″·0 my measures require the mean correction of +0″·12. Therefore those of Kaiser, measured with Airy's micrometer, would be, in the mean, subject to a constant error of 0″·20. Certainly this error is not produced by an uncertainty in the determination of the value of the micrometer-screw, as greater distances do not indicate greater errors in the measures. Hence it follows that it might be attributed to the so-called personal equation of the observer.

On this occasion it may be mentioned that already in 1843 I had made, in company with my father, a series of measures on certain double stars with the héliomètre oculaire, constructed for our refractor by the late M. Steinheil (Descr. de l'Obs. de Poulk. p. 195). Though the results agreed tolerably well with one another and with those obtained by the wire micrometer, we soon gave up that method of observation on account of the distorted images produced by the division of the ocular lens. No doubt to see well is the first condition of a good observation.

Mr. Downing wonders also that, having ascertained the fact that the position-angles come out correct in the mean of the measures made by a great number of observers, I had not acted upon this knowledge and had not had the double stars habitually measured by several observers. In my opinion this proposition is not practical. It seems to have been overlooked that, to ascertain the fact on which the proposition is based, it was first necessary to go through all those experiments with artificial double stars and to work out the introduction. But supposing that it could have been admitted *à priori* that in the mean of a greater number of observers (say 6) the human eye measures the directions with absolute correctness, then 6 astronomers would have had to do with the same instrument the same work that I have done during 40 years. Not to speak of the immense waste of power, produced by employing such a large staff of observers on the same object, it is evident that instead of 6000 results we should have had until now only 1000, or that, to collect the same number of measures, a space of time 6 times longer, *i. e.* a couple of centuries, would have been required. It ought to be noticed also that the fact mentioned applies only to position-angles, and that for distances it is by no means proved. On the contrary the similitude of the curves of correction given at the end of Vol. IX. for the distances measured by several experienced observers with the wire micrometer, seems to indicate that with this apparatus all observers incline to observe the differences too small; and in the case of Prof. Kaiser we come to the same result for the use of Airy's double-image micrometer.

I avail myself of this opportunity to subjoin the following communication. On page 157 of the Introduction to Vol. IX. I have expressed our intention to compute ourselves the definitive corrections to be applied to the published measures according to the investigations laid down in the Introduction. I am glad to be able to inform astronomers engaged in researches on relative motions, that this computation is already performed by M. Dubiago. The corrected coordinates will be published in a couple of months as a supplement to Vol. IX. I am, Sir, yours faithfully,

Pulkowa. OTTO STRUVE.

The Figure of the Earth.

SIR,—

In the article on the Figure of the Earth, in the 'Observatory,' for 1879, January 1, page 313, line 6 from the bottom, there is allusion to "Archdeacon Pratt's theory, that where the crust of the earth is thickest, there it is least dense, &c."

So far as I am aware, this theory was adopted by Archdeacon Pratt, but did not originate with him. The earliest suggestion of it, I believe, is to be found in the 'Philosophical Transactions,' 1855, pp. 101–104.

The laborious calculations of the attraction of the Himalaya mass, and the first inference "that this calculated attraction is far greater than is required to explain the disturbance of latitude," are entirely the work of Archdeacon Pratt.

I am, Sir, yours faithfully,

Royal Observatory, Greenwich, G. B. AIRY.
1879, January 3.

Klein's Crater near Hyginus.

SIR,—

Since the announcement by Dr. Klein of the existence of a crater where one had not previously been seen by him much ambiguity as regards its place and character is found in the published accounts that have come under my notice. Since June 1877, about a month after Dr. Klein first noticed it, from a variety of adverse circumstances, I have been unable telescopically to observe the Moon's surface, and consequently I have not seen the new crater; but I have carefully compared the published drawings, of which I have four now before me. The first published, in order of time, is 'Lunar Chart No. 1' of the Selenographical Society; the second reproduced from *Sirius* in the 'Astronomical Register,' November 1878; the third a key-map from a drawing in my possession by Captain Noble, dated November 2, 1878, published in the 'English Mechanic,' December 13, 1878; and fourth a drawing by Henry

Pratt, dated October 17, 1878, reproduced in the 'Observatory' for January 1879. These drawings differ somewhat considerably *inter se*. To my eye Capt. Noble's drawing appears to convey the best impression of the region in question. I must, however, request the indulgence of the reader under the circumstance of my not having actually seen the locality since June 1877. The distinguishing feature of Capt. Noble's drawing is, that south-west of a line joining Hyginus and the extremities of the two ridges bounding Neison's valley *i* the surface is smooth. As has been already stated elsewhere, Capt. Noble did not see the new crater. The drawing best comparable with Capt. Noble's is Pratt's, taken under the previous sunset on October 17, 1878, at 11^h. Taking Capt. Noble's key-map ('English Mechanic,' Dec. 13, 1878) as our guide (which, by the way, is the only drawing that furnishes a list of the objects depicted), Pratt represents the western half *only* of Mädler's spiral mountain, and in place of Noble's dome-shaped mountain a *semi-crater*, as if the eastern part of the spiral, with its included mountain (*qu.* crater) forming the inner circumvallation, was absent. As this appearance was presented under the setting Sun, renewed and careful observation is necessary to elucidate the differences between Noble's and Pratt's drawings.

There is one remarkable circumstance which characterizes all the observations hitherto published of Klein's crater,—not one observer has fixed its position by *alignment*; for example, by *two* lines intersecting at the crater and bisecting *four* moderately conspicuous objects, two on each side. If Dr. Klein had adopted this simple method of determining the position of his object much of the ambiguity and confusion which Mr. Pratt speaks of would have been prevented.

Capt. Noble gives (β, γ, and δ) a continuous chain, rudely like a cornucopia, of which β is absent in Mr. Pratt's drawing, but replaced by two peaks; γ and δ are very faithfully represented by Pratt.

Neison's valley *i* is well shown on both drawings. Noble has two prominent peaks on the northern boundary shown by Pratt, who gives three low peaks on the southern.

In the area north-west of the south-west arm of the great cleft, which is quite smooth in Noble's drawing, Pratt shows a series of very low curved banks, which he says are "curiously suggestive of waves from Hyginus as a centre of disturbance. Of these I find no traces on the maps." It is very desirable that the existence of these low curved ridges should be confirmed, also what relation they may have to Klein's crater, if it be any thing different from a shallow depression combined with a dark ground marking, as suggested by Mr. Pratt.

The region was observed by Mr. Isaac Ward, of Belfast, on the evening of December 31, 1878, who records a prolongation of Noble's brilliant valley A, of a curved form, towards the south-west, which would pass among Pratt's low curved ridges and probably cut completely through some of them.

I am, Sir, yours faithfully,
January 11, 1879. W. R. Birt.

The Sun's Corona and Meteor-Streams.

Sir,—

Mr. M'Cance's letter touches a weak point of all eclipse observations which have hitherto been made.

We have no decisive information as to the variation of the brightness of the corona, and still less of the intensity of the continuous spectrum. The quotations I have given only prove that the line-spectrum during the last eclipse was very faint compared to the continuous spectrum; as far as I know, nobody has suggested that the continuous spectrum really was brighter. Those who, like myself, had not observed the coronal spectrum before were struck indeed with the brilliancy of the continuous spectrum. But the reason of the surprise is only due to the fact that in reading over former eclipse accounts, one was accustomed to consider chiefly the line-spectrum, and to pass over the continuous spectrum as of secondary importance. Professor Young is the only observer who could give an opinion as to the relative brilliancy of the continuous spectrum during the late eclipse as compared to that which he observed during former eclipses. As far as I know, he has not done so.

I am writing without references; but, as far as I remember, Mr. Stone in 1874 observed the continuous spectrum to a greater distance from the Sun than was done during the late eclipse with instruments of even smaller dispersive power. Mr. Janssen, in 1871, also used an instrument of comparatively large dispersive power, when he discovered the dark Fraunhofer lines. The fact that he could see dark lines at all shows that the spectrum must have had some brightness. I do not mean to say that we have any evidence that the continuous spectrum this year was less bright than on former occasions, but we have no evidence to the contrary.

The question raised by Mr. McCance is complicated by the fact that only a small part of the continuous spectrum is due to light scattered from small particles; the larger part varies independently of the number of particles, and may decrease as this number increases.

I should like to state that I consider the concluding paragraph of my article simply as a suggestion which I believed to be worth discussing. I am by no means prepared to defend it against all objections which might be urged.

The following considerations, however, may help to put some of the questions involved into a clearer light. We observe that certain phenomena on the Sun's surface involving kinetic energy are periodic. Where does this kinetic energy come from? If, as I believe nobody doubts, it is due to a transformation of heat into energy of visible motion and back again into heat, we know it must be attended by the passage of heat from a hot to a cold body. The symmetry of the sun-spot area round the solar equator suggests the idea that it is due to a flow of heat from the equator to the poles. This also, I believe, is generally admitted. But this leads us at once to the conclusion that the solar heat is generated chiefly round the solar equator, and that this generation of heat is periodic. Up to this point we are on absolutely safe ground. It can be proved from well-known principles that the energy of the solar cyclones must have existed either as energy of visible motion or as energy of position; that is to say, either the Sun is contracting or it loses some of its rotatory velocity, or bodies fall into it from the outside. And I ask, is it more likely that the first two causes should act periodically or that the last does? But in every case the periodicity of Sun-spots proves a periodicity of solar heat.

I am, Sir, yours faithfully,

1879, Jan. 14. ARTHUR SCHUSTER.

Markings on Jupiter.

SIR,—

I have read with great interest the remarks on this subject from your correspondent C. W. Pritchett, but should like to make notes against some of them. On July 9, at 11^h the portion of the disk more especially observed on the previous occasion, July 6, was just passed round the W. limb out of view. When the drawing was taken at 12^h 18^m on July 9, the central meridian was nearly 139° distant from that of July 6. At 10^h on July 10 the central meridian of the drawing was 67° distant from the centre of the disk. On July 15^d 10^h the central meridian was 100° E. of that of July 9^d 12^h 18^m. The oval pinkish spot of which mention is made was first noticed by myself with a 9¼ in. With mirror on July 27; but it was not until September that I commenced a series of observations on the planet with any thing larger than 2¼ in. From the beginning of September until the end of November I kept up a constant watch on the planet with a 5¼ in. Calver mirror, using mostly a power of 168. I observed the pink patch on nine occasions between July 27 and Nov. 23.

Between July 27^d 13^h and November 11^d 5^h·5, the longitude of its *p* extremity seems to have increased some 24°, which means a proper motion eastwards of more than 230 miles per day. But that is not all; at 6^h·5 Oct. 25, the oval patch was well elevated in the S. temperate zone, whilst Nov. 11^d 5^h·5 it had seemingly "pushed" the equatorial belt some 8000 miles nearer the equator.

At 11^h on July 27, I fancied the shadow of Sat. II. was elongated E. and W. I was using a 9¼ in. With, with 8 in. stop; powers 132 and 215.

Hoping you will not consider this an intrusion,

I am, Sir, yours faithfully,

Southampton, 1879, Jan. 13. FRANK C. DENNETT.

Jupiter's Second Satellite.

Sir,—

It may interest Mr. Tebbutt to know that a letter on the oversight committed by Sir J. Herschel and others, as to the eclipses of Jupiter's Second Satellite, appeared in the number of the 'Astronomical Register' for May, 1864 (Vol. ii. p. 108). The writer, "G. H.," gives a list of the authors in whose works he has noticed the error in question, and another of those who have not made the mistake.

I am, Sir, yours truly,

1879, Jan. 18. James L. McCance.

NOTES.

Are the Elements Compound Bodies?—Mr. Lockyer's spectroscopic researches in the preparation of his map of the Solar spectrum have led him to the remarkable conclusion that the so-called elements are really compound bodies, and that at higher temperatures, such as we find in the Sun and some of the stars, they are broken up into their constituents. In order to compare the solar spectrum with the spectra of the metallic elements, it was first necessary from the photographs of each of the latter to eliminate the lines due to other elements present as impurities. In the course of the work (which required more than 100,000 observations of about 2000 photographs), Mr. Lockyer found that the hypothesis that the identical lines in different spectra were due to impurities was not sufficient. For in that case, these should have been the longest lines of their respective spectra, *i. e.* those which reach furthest from the electrode, extending right across the spectrum, and which are therefore the most persistent lines. Now a comparison of one spectrum (iron) with another (titanium) showed that whilst the longest lines of titanium were absent in the iron, and that therefore titanium did not exist as an impurity in the iron, there were coincidences of some of the short lines in iron and titanium; and these lines Mr. Lockyer considers to belong to the common base of the two metals iron and titanium. Similar coincidences of short or *basic* lines were found in the case of iron and other metals, *e. g.* uranium, barium, cobalt, thallium, didymium, &c. On this theory of a common base, the *basic* lines, short at first, should become longer and stronger as the temperature is raised to the point at which the so-called element is dissociated, whilst the other lines should gradually fade out. For evidence of this Mr. Lockyer turns to the spectra of the heavenly bodies, in which we have a descending scale of temperature, viz:—(1) the hottest stars (Vega type), showing only H, Ca, and Mg; (2) the Sun, showing H, Ca, Mg, Na, and Fe; (3) cooler stars, showing Mg, Na, Fe, Bi, and Hg; (4) the coolest stars, in which the metals have entered into chemical combination, and only fluted bands of the metalloids are seen.

The spectrum of calcium first engages Mr. Lockyer's attention, This consists mainly of four lines, one in the red, one in the blue, and a pair (the two H lines, or H and K as Mr. Lockyer calls them) in the violet. In the comparatively low temperature of the flame, the red line is the strongest, in the electric arc the blue now takes the first place, whilst in the Sun the lines H and K, faint at the lower temperatures, become the most important in the spectrum, the red line being weak and the blue line only moderately strong. Thus far, however, the changes might, as Prof. Stokes has pointed out, be referred to the law that as the temperature is increased, the proportion of the violet and blue radiations to the red increases also, so that it would not be necessary to suppose that any dissociation has taken place. Now the important photographs of stellar spectra taken by Dr. Huggins give valuable testimony on this point. Dr. Huggins finds that in Altair the line K is only half the breadth of H, whilst in Sirius and Vega K is altogether absent. Thus in the hottest stars the more refrangible of these two lines, which are of equal intensity in the Sun, has altogether disappeared. Again Prof. Young finds "that in solar storms, whilst H has been injected into the chromosphere 75 times, and K 50 times, the blue line at wave-length 4226·3, the all-important line at the arc temperature, was only injected 3 times." "Further, in the Eclipse of 1875, the H and K lins left the strongest record in the spectrum of the chromosphere, while the line near G, in a photographic region of much greater intensity, was not recorded at all."

After discussing the spectra of iron and lithium, Mr. Lockyer comes to that of hydrogen. Under certain conditions of temperature and pressure, Dr. Frankland and he had found, in 1869, that the spectrum reduced itself to the one line F in the blue-green, and that the *h* line was not visible with low battery and Leyden-jar power, and might therefore be looked upon as an indication of high temperature. In connection with this, Mr. Lockyer points out that eclipse observations "show that at the time when the hydrogen lines were most brilliant in the corona, the calcium lines were not detected; next, when, the hydrogen lines being still brilliant, the *h* line was not present (a condition of things which in all probability indicated a reduction of temperature), calcium began to make itself unmistakably visible; and, finally, when the hydrogen lines are absent, they become striking objects in the spectrum of the corona." Again in a photograph of the spectrum of indium, the strongest line is one of the lines of hydrogen (*h*), the other line of hydrogen (near G) being absent, whilst the C line has been observed by eye. Mr. Lockyer adds that he has other evidence to show that the substances which give the D_3 and 1474 K lines are really other forms of hydrogen, one being more simple than that which gives *h* alone, and the other more complex than that which gives F alone. Thus hydrogen at the lowest temperature would show the 1474 K line, with the feeblest spark at lowest pressure the F line, with the spark without jar the C and "near G" lines, with the jar-spark the *h* line, in the chromosphere D_3, and in the Sun these various lines integrated as it were.

DISCOVERY OF BRORSEN'S COMET.—M. Tempel, at Florence, found Brorsen's periodical comet quite close to the horizon on Jan. 14. It was *n.f.* of the nebula of II. Class, Gen. Cat. No. 4900, and was smaller but brighter than it. The position of the nebula is R.A. $23^h\ 3^m\ 32^s$, N.P.D. 119° 12′. The comet has thus been found more than a month earlier than the date at which Prof. Schulze commenced his ephemeris (*cf.* 'Observatory,' No. 20, p. 276), and we may therefore expect that it will be well observed at this return to perihelion.

MR. ELLERY writes that an amateur astronomer in New Zealand finds a bright-line spectrum in γ Argûs.

MR. B. KIDD, writing from Bramley, Guildford, mentions that he saw Klein's black crater pretty well on Nov. 2, though perhaps hardly as clearly defined as Mr. Rand Capron's sketch in last Number.

EDITOR.

Ephemeris of the two Outer Satellites of Uranus (during absence of Moonlight).

Greenwich, midnight.	Titania.		Oberon.	
	Position.	Distance.	Position.	Distance.
1879.	°	″	°	″
Feb. 14	261·2	8·5	198·3	43·2
15	205·1	25·9	191·9	46·5
16	193·3	34·9	185·0	40·4
17	182·2	27·5	172·9	26·6
18	138·7	9·8	126·2	11·7
19	33·2	19·7	43·9	19·5
20	16·8	33·5	24·7	35·3
21	6·7	31·9	16·5	45·0
22	345·2	16·1	9·1	45·8
23	229·7	13·0	2·4	37·4
24	200·9	30·1	346·3	22·3
25	190·4	34·4	276·8	11·1
26	176·3	22·7	216·6	24·1
Mar. 9	22·0	29·3	355·1	29·9
10	10·8	34·5	321·2	14·3
11	356·9	23·8	233·9	16·7
12	282·1	8·6	207·8	32·5
13	208·2	23·9	198·0	43·6
14	194·4	34·5	191·1	46·2

A. M.

Astronomical Memoranda, 1879, *Feb.* 14–*March* 15.

Sun. Feb. 14, sets 5^h 10^m, rises 19^h 17^m; March 16, sets 6^h 3^m, rises 18^h 12^m.

Equation of time:—Sun *after* clock, Feb. 14, 14^m 25^s; Feb. 24, 13^m 27^s; March 6, 11^m 30^s; March 16, 8^m 51^s.

Sidereal Time at Mean Noon:—Feb. 14, 21^h $36^m{\cdot}5$; Feb. 24, 22^h $16^m{\cdot}0$; March 6, 22^h $55^m{\cdot}4$; March 16, 23^h $34^m{\cdot}8$.

Moon. New, Feb. 20, 16^h; First Quarter, Feb. 28, 20^h; Full, March 8, 1^h; Last Quarter, March 14, 16^h.

Occultations:—Feb. 26, 7^h 45^m 26 Arietis Oc. D. 104°; March 2, 4^h 52^m 139 Tauri Oc. D. 65°; March 3, 9^h 19^m ω Gemin. Oc. D. 113°; March 5, 11^h 58^m o^2 Cancri Oc. D. 353°; March 9, 9^h 11^m B. A. C. 4201 Oc. R. 251°, 12^h 17^m *q* Virginis Oc. R. 224°. The angles at disappearance (D.) and reappearance (R.) are reckoned from the *apparent* N. point towards the right hand round the Moon's circumference as seen in an inverting telescope.

Mercury is a morning star till March 4, when he is in superior conjunction. On March 15 he sets in the W. 1^h after the Sun.

Venus is an evening star, setting 1^h 32^m after the Sun on Feb. 14, and 2^h 13^m after on March 15. Her disk is gibbous, the illuminated portion being 0·959 on Feb. 14, and 0·916 on March 15, and the corresponding diameters $10''{\cdot}3$ and $11''{\cdot}0$.

Mars is a morning star in the S.E., advancing through Sagittarius.
Feb. 14, R.A. 18^h $29^m{\cdot}3$, Dec. 23° 44′ S., tr. 20^h 52^m, rises 17^h 1^m.
Mar. 16, R.A. 20 2 ·9, Dec. 21 22 S., tr. 20 27 , rises 16 20 .

Jupiter is a morning star, too near the Sun for observation.

Saturn is an evening star, midway between β Ceti and γ Pegasi.
Feb. 14, R.A. 0^h $6^m{\cdot}8$, Dec. 1° 39′ S., tr. 2^h 30^m, sets 8^h 25^m.
Mar. 16, R.A. 0 19 ·7, Dec. 0 13 S., tr. 0 45 , sets 6 48 .

Uranus is in opposition on Feb. 20, midway between Regulus and ρ Leonis.

Neptune is an evening star, midway between α and β Arietis and α Ceti.

Publications received:—H. Geelmuyden, *Om Zodiakallyset* (Kristiania, 1878)—Prof. Loomis, *Contributions to Meteorology* (Amer. Journ. 1879, Jan.)—Antoine d'Abbadie, *Instruments à employer en voyage et manière de s'en servir* (Paris, 1878)—Prof. Winnecke, *Ueber einen von Scheiner im April* 1612 *in der Nähe von Jupiter beobachteten veränderlichen Stern*—C. Todd, *Adelaide Meteorological Observations*, 1878, *Feb., March, April*—J. C. Houzeau, *Annuaire de l'Observatoire Royal de Bruxelles*, 1879—*Sirius, Zeitschrift für Populäre Astronomie*, 1879, *Jan.*: Edited by Dr. Hermann J. Klein (Leipzig, Karl Scholtze).

EDITOR.

THE SOLAR SPECTRUM NEAR D.

(From Micrometer Measures.)

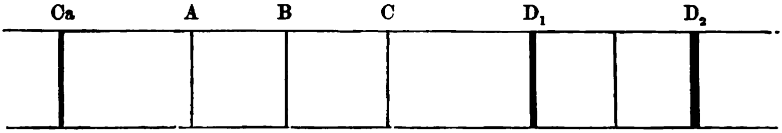

Fig. 1. Air-spectrum at noon on Oct. 19th. A, B, and C are the most persistent of the air-lines, if, indeed, they are air-lines.

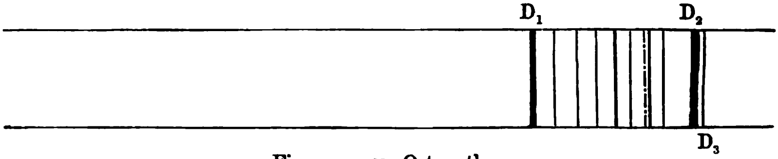

Fig. 2. 4 P.M., Oct. 19th.

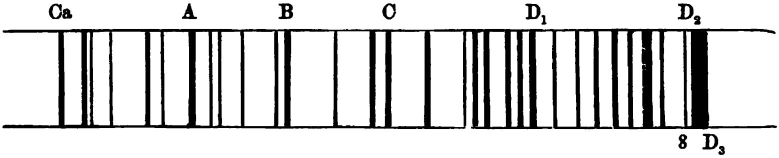

Fig. 3. Air-spectrum lines between Ca and D_2 at 5.20 P.M., Oct. 19th. Line 8, between D's, discovered 5.45 P.M., 19th Oct. 1878. D_2 and D_3 appeared as shown at 6 P.M.; all the thickening seems to be in D_3. Dotted line shows the position of a bright streak.

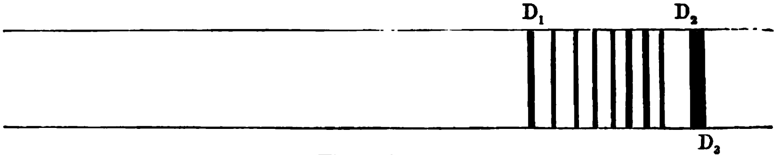

Fig. 4. Sunset.

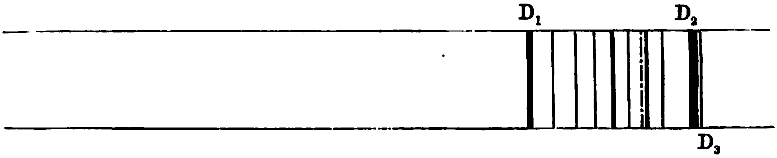

Fig. 5. Air-lines between D's as usually seen at Sydney at noon. Dotted streak is a bright line seen at Sydney, Oct. 6th, 1878.

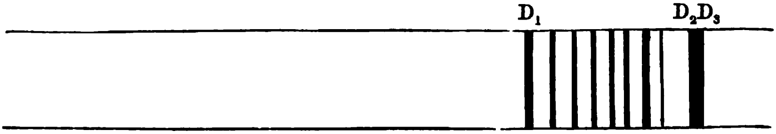

Fig. 6. Sunset.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 23. MARCH 1. 1879.

ANNIVERSARY MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1879, February 14.

LORD LINDSAY, M.P., F.R.S., &c., *President*, in the Chair.

Secretaries: Mr. J. W. L. GLAISHER, F.R.S., and
Mr. A. COWPER RANYARD.

THE Minutes of the last Anniversary Meeting having been read and confirmed, the President put it to the Meeting that Reporters be admitted as at Ordinary Meetings, which was carried unanimously.

The Astronomer Royal. May I take this opportunity of offering for the acceptance of the Society a photograph of a portrait of the late Miss Herschel, for which we are indebted to the kindness of Lady Herschel? I believe it will be found to be a very good photograph of a very good painting.

The President. I will ask the Astronomer Royal to convey to Lady Herschel the heartfelt thanks of the Society for a photograph which I hope will always remain in the Society to remind them of one of their most valued Associates, one of the two lady Associates of our Society.

The President. I believe it will be in accordance with the desire of most of the Members present that we should take the special business before the mere ordinary formal business of the Meeting; and therefore I will read to you a circular which has been sent to most Fellows of the Society. (*The Circular having been read*) The resolution which I beg leave to put to the Meeting is, "That this Meeting approve and concur in a provisional agreement made between the Council and Mr. Edward Lee for the conveyance and release to Mr. Lee of all the right and claim of the Society to the advowsons of Stone and Hartwell in consideration of the sum of £700."

Sir Edmund Beckett. As a matter of form I beg to second that.

Mr. Freeman. I should like to be informed by the Secretaries as to the date of the private Act of Parliament by which the manors of Hartwell and Stone were entailed.

Mr. Ranyard. I think it is 1832.

Mr. Freeman. It is quite sufficient if I know that it is certainly previous to 1844.

Mr. Glaisher. It is certainly previous to that.

Mr. Freeman. May I ask what is the net annual value of the livings of Hartwell and Stone respectively?

Sir Edmund Beckett. Mr. Merriman, who has investigated the matter, has told us that the net annual value of Stone would not be above £350; consequently we have got two years' purchase. As for the living of Hartwell we have no title to it, so that it is of no use talking about it.

Mr. Freeman. We seem to be selling the living of Stone for a very moderate price to Mr. Edward Lee; but still I quite concur in the view that it is undesirable that this Society should have any litigation on the matter. No doubt we owe a great deal to the memory of Dr. Lee, and under the circumstances we ought not to make a hard bargain; nor will Mr. Edward Lee desire to make a hard bargain with the Society. I am very happy to find that the Council have taken that action which the circumstances of the case seem to demand.

Rev. Mr. Heaven. I have great pleasure in supporting the resolution. Having been curate to Dr. Booth for the last sixteen months, and being a Fellow of the Society, I think I ought to know as much about the circumstances of the case as most of the gentlemen present; and I can only say that I think the Society do well to get out of the living of Stone at the value which has been put down for it. The house is in a very dilapidated condition, and sooner or later it would have to be rebuilt by the Society. The church also has been partially restored within the last twenty or thirty years, but its roof is in a very dilapidated condition. Therefore I think you do well in disposing of the living, especially as, if you go into litigation with Mr. Lee, you will have to pay more than £700 in law expenses; therefore I think the Society are well advised in accepting the terms agreed upon.

The Resolution was put to the Meeting and carried unanimously.

Mr. Gill. I beg to propose that the President's Address be now read before we receive the formal Report of the Council.

Mr. Neison. I beg to second that.

The President then delivered his Address on presenting the Gold Medal awarded by the Council to Prof. Asaph Hall for his discovery and observations of the Satellites of Mars and for his determination of their orbits. He commenced by alluding to some of the work which had rendered Prof. Hall's name already familiar at the time he announced his great discovery to the world:—An examination of his writings would show that there are few departments of astronomy to which he has not paid some attention, combining the skill of the Observer with the labours of the Mathematician. From 1858 to the present time, almost every volume of the 'Astronomische Nachrichten' contains notices of his zealous observation of minor planets and comets, together with elements and ephemerides of many of them.

The early history of Mr. Hall affords a bright example of what perseverance and determination may effect in overcoming even the most adverse circumstances. Left an orphan at the age of thirteen, he was reduced to work for his living, and served for some time as an apprentice to a carpenter, devoting his leisure hours to the study of geometry and algebra. In 1856 he studied one term under Brünnow at Michigan University, and then joined the Cambridge Observatory, where he determined to make himself so useful that they would not care to let him go. In 1862 he was appointed one of the Aids in the Washington Observatory, and in the year following was made Professor of Mathematics. In 1863 he published a memoir on the Solar parallax from Mars observations at Upsala, Santiago, and Washington, deducing the value 8″·84. This was followed by a memoir on the positions of Fundamental Stars, a special problem on which he offered some very sound suggestions. In 1868 he made a remarkable observation of an occultation of Aldebaran in full daylight, when the star was only 8° 12′ from the Sun's centre, and the Moon itself was invisible. He also observed the Solar Eclipses of 1869 and 1870. In 1870 he published in the 'American Journal' an interesting paper on the Secular Perturbations of the Planets, and the next year one on the Astronomical Proof of a Resisting Medium in Space, in which he shows, from the investigations of Möller and Oppolzer, that the comets of Faye and Winnecke do not by their motions give any indication of the presence of a resisting medium such as Encke assumed to exist. It is possible, however, that a resisting medium increasing in density in the neighbourhood of the Sun would account for the anomalies in the motion of Encke's comet without affecting the other two, which have a much greater perihelion distance. The most interesting paper which Prof. Hall has communicated to the 'Monthly Notices' is one in Vol. 33 on the Determination of Longitudes by Moon culminations, in which he shows that such determinations are liable to large constant errors.

Several other papers by Prof. Hall were alluded to by the President, and in particular his determination of the period of rotation of Saturn by observations of a bright spot upon the ball, extending from 1876, Dec. 7, to 1877, Jan. 2, and an investigation of the form of the shadow of the ball of Saturn on the ring, which, as he points out, may in a special case be sensibly a straight line*.

Passing on to the important discovery for which the Council awarded Prof. Hall the medal, the President referred in a few words to the unsuccessful searches for the Satellites of Mars which had been made previously to 1877:—In 1830 Mädler searched in vain with a refractor of 3¾ inches aperture for a satellite, and came to the conclusion that such a satellite, if it possessed the same reflecting power as its primary, could not have a diameter exceeding 23 miles, as, if larger, it could not have escaped discovery. Prof. D'Arrest in 1864 made a search which was again unsuccessful, and, in a paper in the Astron. Nachr., he showed that an elongation of 70′ would give the satellite a period greater than that of Mars round the Sun, from which he drew the inference that it is useless to search for a satellite at a greater distance than 70′. There is another note on this subject, which seems to have escaped Prof. Hall's researches. It is in 'Stjernehimlen,' by F. Kaiser, translated into Danish by Mathilde Oersted, with a preface by D'Arrest, and runs thus:—"That up to the present time no satellite of Mars has been found, may possibly be owing to the fact that such satellite, from the nature of the case, must be very near to its primary, and that any faint star becomes totally extinguished and rendered invisible in the immediate neighbourhood of the brilliant planet. Formerly it was the custom to explain the same fact by the contrary reason, viz. by pointing out how difficult it must be to find a satellite of Mars at a possibly great distance from the planet. However, by reason of the planet's small mass, this can by no means be the case."

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* *Cf.* Observatory, No. 7, p. 218.

Notwithstanding the failures of distinguished observers, Prof. Hall, encouraged by his wife, commenced the search, early in August 1877, for a satellite at a considerable distance from the planet; but as all the faint objects examined turned out to be nothing but stars, he began on Aug. 10 to examine the region close to the planet, and within the glare of light which surrounded it, keeping the disk of Mars just outside the field of view. The next night, August 11, at $14^h\ 30^m$, Prof. Hall discovered the outer satellite, but, on account of bad weather, it was not till August 16 that he demonstrated clearly that it was moving with Mars. On the night following (August 17), while waiting for and watching the outer satellite, Prof. Hall discovered the inner one; and the last doubts as to the character of the faint objects thus found having been dispelled by the observations of August 17 and 18, the discovery was officially announced by Admiral Rodgers. The appearances of the inner satellite were most perplexing, and to quote Prof. Hall's words:—"Still for several days the inner moon was a puzzle. It would appear on different sides of the planet on the same night; and at first I thought that there were two or three inner moons, since it seemed to me at that time very improbable that a satellite should revolve around its primary in less time than that in which the primary rotates." To set this point at rest, Prof. Hall watched the satellite throughout the nights of Aug. 20 and 21, and was satisfied that there was but one inner moon, which performed its revolution in less than one third of the time of the rotation of Mars, a case unique in our solar system.

Such, in a few words, is the history of a great astronomical discovery; and it is satisfactory to think that the United States Government has received a reward already for the enlightened munificence displayed in placing so magnificent an instrument in the hands of one so capable of using it.

The labours attending this search by no means ceased with the attainment of the object in view; for Prof. Hall, not merely content to publish his crude observations, has reduced them, and from the results has given the elements of the orbits of the satellites.

The President then proceeded to describe the methods of observation and computation as detailed in Prof. Hall's memoir (of which an abstract was given in the 'Observatory,' No. 19), referring particularly to his determination of the mass of Mars, which might be considered as the most important outcome of his brilliant discovery.

The President continued :—"Various estimations of the brightness of the satellites have been made, ranging from the eleventh to the fifteenth magnitude in the case of Deimos. With respect to Phobos, much uncertainty has been expressed, but at the same time the opinion seems to be that it is rather brighter than the outer satellite. Your medallist mentions having, on Aug. 17, 1877, observed a fixed star which was mistaken for Deimos; and thinking that a determination of its magnitude might be of interest to the Society, I requested Dr. Copeland, if possible, to observe it, and I have received the following report :—'Jan. 16, 1879. Altitude of star 13° ±. I noted Hall's star as about 11½ mag., not seen in finder. Stars about 1^m brighter I could see well in the finder. In the 15¼ inch the star bore illumination of wires well.' The place was roughly determined and agrees with Prof. Hall's place within $1^s.5$ and $1'.1$. Since writing this, I have received another observation of the star, in which the observation note runs thus :—'Hall's star is quite 11th mag.; could be very well seen when running along the bright wire which was very well illuminated with the maximum brightness.' Your medallist states that this star was a little brighter than Deimos at the time he observed it. The above observations are therefore confirmatory of this estimation.

"Prof. Hall closes his report by giving data for the computation of Ephemerides of the Satellites for the present year, when Mars will have a comparatively favourable declination, 18° north. The estimated brightness on Oct. 10 will be 0·63, and on Nov. 4 0·73, the unit being the brightness on Oct. 1, 1877; and your medallist concludes by giving examples of the computation, using the table prepared for this purpose.

"And now, Mr. Ranyard, may I request you, as acting for the Foreign Secretary of the Society, to place this Medal in the hands of the Minister of the United States, to be transmitted to Prof. Asaph Hall as the highest mark of esteem in the gift of the Royal Astronomical Society? Assure him at the same time of the deep interest that we in England have ever felt in watching the progress of our beloved science, under the able generalship of our cousins in the far West."

Mr. Ranyard gave a summary of the Report of the Council for the last year :—

The number of Fellows had increased from 580 to 590, and of Associates from 37 to 40. The Council had to record the loss by death of Padre Secchi, Capt. Bailey, E. B. Beaumont, J. Bonomi, Rev. J. Booth, J. Carter, W. R. Cooper, Solomon Moses Drach, H. S. Ellis, Rev. C. Gribble, Rev. P. Holmes, H. Johnson, John Matheson, Rev. R. Main, H. Mugridge, J. W. Nichol, G. G. G. F. Pigott, G. V. Vernon, Sir A. S. Waugh; and obituary notices were given of Padre Secchi, Rev. R. Main, Joseph Bonomi, and others.

The Treasurer's Report showed that the Receipts for the past year were £1545 (exclusive of a Government Grant of £250, on account of Mr. Gill's expedition to Ascension), whilst the Expenditure was £1426. A sum of £660 had been spent on Printing, and £192 on the Library.

The increasing usefulness of the Library was shown by the circumstance that the number of books taken out had increased 20 per cent., and that the number of Fellows making use of it had also increased considerably.

Reports had been received from various Observatories, giving an account of the work done during the past year.

The following points in the Progress of Astronomy were noticed:—Prof. Newcomb's Researches on the Moon's Motion; Mr. Hill's Researches on the Lunar Theory; Mr. Darwin's Investigation of the Influence of Geological Changes on the Earth's Axis of Rotation; Mr. Peirce's Photometric Researches; Schmidt's Map of the Moon; Lohrmann's Map of the Moon; Discovery of Minor Planets and of Comets; Transit-of-Venus Reductions; Elements of Vulcan; and a very valuable and exhaustive Report on the Progress of Meteoric Astronomy during the year 1878 by Prof. A. S. Herschel.

Mr. Common. I rise to propose that the Report which we have now heard read be received and adopted, and that it be printed and circulated in the usual manner, together with the President's Address.

Mr. Freeman. I beg to second that resolution. I would observe that we seem to be rather in arrear with our 'Monthly Notices.' I have only this morning received the report of the meeting in January, and I have had no report as yet of the meeting in the early part of this month. I suppose there must be a distinct report of what happened then.

The President. I think that complaint had better be made to the Post Office.

Mr. Ranyard. The January meeting was the last one that was held. The No. of the Monthly Notices was issued a week ago; and it contains some papers which stood over for some time. It is always difficult in making up the sheets to get all the papers in.

Mr. Freeman. There is no doubt some difficulty; but I think one month after date is a very long time to wait.

The President. Did I understand Mr. Freeman to say that he seconded the proposition?

Mr. Freeman. Yes; I seconded the proposition. I only offered a few remarks which I thought might be useful.

The resolution was carried unanimously.

The President. I will now call upon one of the Auditors to read their Report for the past year.

Mr. Esdaile, the senior Auditor, read the Report to the effect that having examined the Treasurer's account and the amount of the assets and property of the Society, they found the same to be correct; that the receipts and expenditure for the past year were correct according to the Treasurer's statement; that the balance in hand on the 31st of December, 1878, amounted to £630 8*s.* 11*d.*; that they found the property of the Society in a satisfactory state,—the books and instruments and other effects having, as far as possible, been examined and found in a satisfactory condition; and that they had laid upon the table a list of the Fellows who were now in arrear for sums due at the last Annual General Meeting.

The President. The next business is to ballot for the officers for the ensuing year, and to appoint scrutineers; and I would request Mr. Brewin, Mr. Lecky, and Mr. Heaven to be so good as to act as scrutineers.

Mr. Barrow. I do not know whether I am too late; but there was an observation in the report which the Auditors have just presented in which I venture to think they are a little out of form, and they do not adopt the precedent of past years. I think that the list of the names of those Fellows who are in arrear ought not to form a part of their report, because we do not usually publish it. It is a separate list, and ought not to be included in their report, our object of course being to avoid printing and circulating the names of any gentlemen who are in arrear.

The President. You will find that the report says that the Auditors have laid on the table a list; but it does not, I imagine, necessarily follow that it is for publication.

Mr. Brewin, Mr. Lecky, and Mr. Heaven were appointed scrutineers by the unanimous vote of the Meeting.

The following were balloted for and duly elected Fellows:—John Marshall and R. Palmer Thomas.

The ballot for Officers and Council was then proceeded with, and resulted in the following list:—*President*: Lord Lindsay. *Vice-Presidents*: Prof. Adams, Sir G. B. Airy, William Huggins, William Lassell. *Treasurer*: Francis Barrow. *Secretaries*: J. W. L. Glaisher and A. Cowper Ranyard. *Foreign Secretary*: J. R. Hind. *Council*: Sir E. Beckett, Prof. Cayley, A. A. Common, E. Dunkin, D. Gill, E. B. Knobel, G. Knott, A. Marth, E. Neison, Capt. W. Noble, H. Sadler, Capt. G. L. Tupman.

The Inventor of the Telescope.

No instrument has so wonderfully forwarded our knowledge of the secrets of Nature as the telescope. Its invention changed the whole science, and problems were solved which no one had ever thought of. By its aid we have discovered the nature of the heavenly bodies, have been able to understand their movements, and on it is founded our insight into the laws that govern the universe. It is therefore only natural that the endeavour has often been made to ascertain to whom we owe so useful an apparatus; but up to nearly half a century ago there reigned much uncertainty about this, and even in our days the handbooks do not agree as to the name of the inventor.

It has been supposed that the telescope was already known to the ancients, and was referred to by Greek philosophers. When, for instance, Democritus says that the Milky Way is a condensed mass of small stars, or when Seneca thinks that there are many more planets than those known in his time, it was thought that such correct notions could not have been obtained without telescopes; but we need only allude to the results that have been obtained in the way of philosophical speculation by such men as Kant or Schopenhauer to show that this argument is not worth much.

An old manuscript, in which Ptolemy is represented with a long tube in his hand, at one time gave strength to this hypothesis; but here reference is made of course only to the tubes, formerly so common, that were used for excluding the side rays in looking at the stars or other distant objects, and in which there were no glasses. The use of such tubes is very old. We find them represented on old Eastern sculptures, sometimes of the same form which in our days the terrestrial telescope has.

It is certainly astonishing at what results the old nations arrived with their insignificant means; but we must take into account the southern situation, the transparency of the sky, and the excellent sight of those that made the observations. The Japanese represent Jupiter with two moons; but there are among ourselves people who can see these with the naked eye, particularly when two are near together. It has been thought that the Indians represented Saturn by a ring; but this might mean a crown. It has also been stated on more than one occasion that Mercury was seen on the Sun; but then at the time the solar spots had not yet been discovered. We possess old star-catalogues of surprisingly rich contents; but Kepler's teacher, Möstlin, could see fourteen stars in the Pleiades, of which others discerned only six *. We may be sure that an author like Pliny would not have omitted to mention it, even among other mere curiosities, if there had been any tradition abroad in his day concerning telescopes. Besides, the Chinese, if ever they had had telescopes, would have preserved the tradition. On mere fables we are hardly warranted in founding any hypotheses; and some savants, as Dutens and Paschius, have gone too far, not to mention those who thought that the devil made use of a telescope when he showed Christ all the kingdoms of the world and their glory.

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* Since this paper was written *this well-known fact* has been referred to in the No. for January, page 282.

In the middle ages several authors might seem to hint at telescopes in some remarks. Thus Baptista Porta, in his 'Magia Naturalis,' 1561, mentions lenses with the words "si utrumque recte componere noveris, et longinqua et proxima clara videbis." He tells us that he communicated his discovery to those of his friends that were either near-sighted or far-sighted, and that they were well served thereby. Of course he only alluded to common spectacles, which were used in the thirteenth century, if not before; but it is hardly possible to understand him on account of his mysterious language. Already Fracastor and Cabæus had experimented with lenses laid one on the other; and Digges's claims to the invention may amount to no more.

Roger Bacon (1214–1294) has greater claims, though he hardly possessed more than the usual lenses. The following are the most important of the remarks that have been adduced in favour of his knowledge of the telescope. He says that Cæsar from the French coast, through an optical tube, observed the things that took place on the coast of England*, and he says that a knowledge of optics is required in making astronomical instruments. Of more consequence is the following passage of his 'Opus Majus' (Edit. Venet. 1750, p. 269):—"Greater things may be performed by refracted light, for from the foregoing principles follows easily that the greatest objects may be seen very small, the remote very near, and *vice versâ*. For we can give transparent bodies such form and position with respect to the eye and the object that the rays are refracted and bent to where we like, so that we, under any angle, see the objects near or far, and in that manner we can at a great distance read the smallest letters, and we can count atoms and sand-grains on account of the greatness of the angle under which they are seen; and, on the other hand, we can make the greatest bodies nearly invisible on account of the smallness of the angle of vision, for the distance has no influence on such observations, only by accident, but the magnitude of the angle of vision. Thus a boy can be seen as a giant and one man as a whole mountain, and be of any size, because we can see a man under as great an angle as a mountain and as near as we wish. And thus a small army can appear very great, a far distant one very near, and *vice versâ*. We can in the same manner bring the Sun, the Moon, and the stars further down, and to be visible over the heads of our enemies, and many similar things, so that a person who did not know the truth would not be able to understand it."

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* A similar tradition exists about Ptolemæus Evergetes.

We are told that Bacon, on account of his knowledge of natural philosophy, and particularly on account of the optical tube he made use of, was looked upon as a sorcerer by his ignorant contemporaries, who in his old age repaid his magnificent labours with fourteen years' imprisonment. It is certain, however, that he did not actually possess a telescope, as he in this case would have made discoveries in the sky; but it is not impossible that, in these dark ages, a great and ingenious idea should have been lost to science; and this point will hardly ever be cleared up. Roger Bacon was profoundly versed in Arabian learning, from which he derived the germs of his philosophy; but, like all the great men of old, he spent much time on researches in astrology and alchemy *. We are therefore not entitled to believe that telescopes were known before the commencement of the seventeenth century, when they appear in the writings of contemporary authors, though these do not agree as to the person to whom the invention is due.

The Capucin friar, Schyrlæus de Rheita, who invented the terrestrial, after Kepler had constructed the astronomical telescope, tells us, in his work 'Oculus Enoch et Eliæ, seu radius sidereo-mysticus,' 1645, that the telescope was invented in 1609 by a Dutch spectacle-maker, Lippensus, elsewhere called Jan Lapprey or Hans Lippersheim, who once looked at the same time through both a convex and a concave lens, which he happened to keep at the right distances from the eye. He found then that a neighbouring weathercock looked larger and plainer than with the naked eye. Therefore he fixed the glasses in a tube. The Marquis of Spinola, who was stopping in Hague, heard about the invention, bought the instrument, and offered it to the Archduke Albert of Austria, then Spanish governor in Belgium. Thus the matter became known to the States, who forced the inventor to sell them an instrument and promise not to make or sell more in future. It has indeed been proved by Van Swinden that Lapprey has good claims to be considered the inventor; but in the foregoing explanation there must be an error, because it has been pointed out that the Marquis of Spinola had already left the country in the autumn of 1608. Hieronymus Sirturus (De Telescopio, 1618) and Lahire (Mém. de l'Acad. Royale, 1717) mention the invention in the same way.

Descartes says, in his 'Dioptrics,' 1637:—"This invention, as illustrious as it is useful, is, to the shame of our science, due to chance and mere trial. About thirty years ago there lived in Alkmaër, in Holland, a certain Jacob Metius, who had never studied, though both his father and his brother were Professors of Mathematics. He found his greatest pleasure in making burning-glasses

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* The mysterious speculations of the astrologers were founded on the belief that heaven and earth reflected the same idea, the whole of nature being connected through every single phenomenon. Modern hypotheses remind one forcibly of these speculations, when famines, bank failures, and depression of trade are thought to be connected with the cycle of the solar spots, and this again with the revolutions of the chief planets.

and mirrors; and when he was thus once in possession of a lot of glasses of different forms he happened to look at the same time through two glasses, of which the one was a little thicker in the middle than on the edge, and the other thinner in the middle. He afterwards fixed them in a tube, and in that way originated a telescope, from which all the later ones have been made, for, as far as I know, nobody as yet has sufficiently explained what form these glasses by right ought to have."

The real name of this Metius, whom also Schott and Harsdörffer mention, was Jacob Adriaanz, and he was the fourth son of Adrian Anthoniezoon, the well-known mathematician and engineer in Alkmaër, who determined the relation between the circumference and the diameter of the circle π to be $\frac{355}{113}$, which up to his time had been assumed, according to Archimedes, to be $\frac{22}{7}$. Jacob, who died about 1630, was an ignorant and eccentric person, but his three older brothers were distinguished mathematicians. The oldest but one, Adrian, was one of Tycho Brahe's many pupils. He died in 1635 as Professor of Astronomy in Franeker. It was he who, on account of his assiduity in studying mathematics, got the surname Metius, which was afterwards assumed by the whole family. His collected works have been published by Bleauw, the famous geographer, another pupil of Tycho Brahe.

Owing to these conflicting statements great uncertainty about the true inventor reigned, until the learned Dutch philosopher Van Swinden cleared up the question by aid of documents which he found in the Dutch libraries. He died shortly after (1823); but his researches were published in 1831 by Moll, of Utrecht. Among these manuscripts is the copy of a petition sent by Metius to the States on the 17th of October, 1608: after assiduous investigation he had two years before invented an instrument through which remote objects were distinctly to be seen. The one he herewith laid before them was made of bad material, and destined merely for trial; but in the opinion of his excellency (Prince Moritz) and others, who had compared both instruments, it was just as good as the one a citizen in Middelburg a short time ago had delivered. He hoped still to be able to improve the instrument greatly, but asked for a privilege according to which everybody who had not made the invention before, and executed it, was to be forbidden, on pain of confiscation and a pecuniary fine, to buy or sell any such instrument during twenty-two years. Besides, he asked for a suitable reward.

The States invited the petitioner to improve the instrument, and promised then to reconsider the case; but Jacob Metius took this answer in bad part, and was not heard of after. It is said that he allowed the prince and one of his brothers, and that only once, to look through the tube; but his brother Adrian, who, however, was never allowed to look through it, speaks well of the instrument, and attributes to him the same discoveries as Galileo made.

The above-mentioned citizen in Middelburg, who already had invented the telescope, was Jan Lapprey, who was born in Wesel, but subsequently established himself as spectacle-maker in Middelburg. He solicited the States, as early as the 2nd October, 1608, for a patent for thirty years, or an annual pension for life, for the instrument he had invented, promising then only to construct such instruments for the government. After inviting the inventor to improve the instrument and alter it so that they could look through it with both eyes at the same time, the States determined, on the 4th October, that from every province one deputy should be elected to try the apparatus and make terms with him concerning the price. This committee declared on the 6th October that it found the invention useful for the country, and had offered the inventor 900 florins for the instrument. He had at first asked 3000 florins for three instruments of rock-crystal. He was then ordered to deliver the instrument within a certain time, and the patent was promised him on condition that he kept the invention secret. Lapprey delivered the instrument in due time. He had arranged it for both eyes, and it was found satisfactory; but they forced him, against the agreement, to deliver two other telescopes for the same money, and refused the patent because it was evident that already several others had learned about the invention, which no doubt refers to the petition they had since received from Metius. On the 13th February, 1609, Lapprey got the remainder of the money after he had delivered the three instruments all right.

The French ambassadors endeavoured to obtain a telescope from Lapprey, but did not succeed, because he would not break his word to the States. But a soldier of Prince Moritz's army traced out the secret, and learned to construct as good telescopes as the inventor. The ambassador sent this soldier, in December 1608, to Henri IV. and Sully. On the whole the invention spread rapidly, and we know several who a year or two after constructed telescopes.

Among these were Simon Marius, in Gunzenhausen, and Zacharias Jansen, the latter of whom for a long time was looked upon as a genuine inventor. Pierre Borel, physician and mathematician to the king of France, claimed the invention for him in his work 'De vero Telescopii inventore,' 1655. He refers to a number of witnesses; but the evidence they gave before the magistrates does not agree either as to the time or as to the name. The son of the supposed inventor says that in the years of his childhood he always heard his father mentioned as the inventor of the telescope, and he says that he had constructed a small one in 1590, when the son was only five years old. His aged sister attributes also the invention to him, but does not remember the time. As far as could be gathered from her evidence, however, Jansen did not make telescopes till after 1610. Two other witnesses speak for Jan Lapprey without any mention of Jansen, and another witness knows merely that the inventor was called Hans, which is the christian name both of Hans Lippersheim and of Hans Jansen. The book contains besides a letter from William Boreel, the Dutch ambassador to the French Court, who was born in 1591 in Middelburg, and had been on intimate terms with Zacharias Jansen, whose whole family he therefore knew. He says that Zacharias's father, Hans, had first invented the compound microscope, and presented one specimen to Prince Moritz and another to Archduke Albert. The latter he saw in 1619 in England at the house of Cornelius Drebbel, of Alkmaër, mathematician to King James, who had got it presented from the Archduke. He tells us further that Hans and Zacharias invented the telescope in 1610, and sold it secretly to Prince Moritz, who wanted to use it in the war in which the States were just then implicated. Meantime the invention became known, and an unknown person, who came to Middelburg, went to Jan Lapprey, thinking that he was the inventor. Lapprey was an intelligent man, and gathered from his remarks sufficient information about the construction of the telescope, and many people believed him to be the inventor, as he was the first who sold telescopes and made them known. Shortly after, however, the mistake was discovered, and Adrian Metius, and afterwards, in 1620, Drebbel, who came to Middelburg, went to Zacharias Jansen and bought telescopes. This Drebbel was a well-known quack at that time, who falsely ascribed this and other inventions to himself, and even as late as 1622 in France was supposed to be the real inventor, as appears from Peiresc's letters.

Galileo states himself that he, in 1609, in Venice heard that a Dutch artist had constructed an instrument through which remote objects were distinctly seen. He then returned to Padua, and invented there, by pure speculation, in the course of twenty-four hours a telescope of the Dutch construction. This cannot possibly be so. The analysis was at the time far from being so developed that such an *à priori* discovery was possible, and Galileo's greatness is moreover not founded on mathematical genius. According to one of his biographers he procured first a number of lenses of different kinds. Already in 1609 the telescope was known in northern Italy, as can be seen from many letters written at that time, of which some even directly mention Galileo's usurpation. Thus G. Fuccari writes to Kepler:—"Galileo wants to be considered the inventor of the telescope, though he, as well as I and others, first saw the telescope which a certain Dutchman first brought with him to Venice, and although he has only improved it very little." Another says that Galileo had got 1000 florins for life for an instrument like that which was sent to Cardinal Borghese from Flanders. Galileo's first telescope magnified only three times; but by perseverance he was at last able to bring the magnifying-power up to thirty diameters.

Fontana's claims are equally unfounded. He asserts that he invented the telescope in 1608, and produces the evidence of two other Jesuits, which, however, merely shows that he was in possession of a telescope with two convex glasses in 1614 and 1621. Fra Paolo Sarpi, who was so cruelly persecuted in his old age, until he died in 1623 in Venice, is mentioned as inventor both of the telescope and the thermometer; but he is said not to have been in possession of these instruments before 1617.

Lastly an author must be mentioned by whom it has been thought that the telescope is referred to, and certainly not without cause. In a work 'De radiis visus,' by Antonio de Dominis, Archbishop of Spalatro, the path which a ray of light follows when it passes through a convex and a concave lens placed at a certain distance apart is described; but it must be remembered that this work, though said to be written long before, was not published till 1611, at a time when telescopes already commenced to be common in Europe; and, moreover, the editor remarks that he had been permitted to add a few chapters.

There is therefore nothing to show that the telescope was invented by anybody else than Jan Lapprey (Hans Lippersheim), a distinguished man, who, moreover, was the inventor of the binocular. Hans Jansen appears to have invented the compound microscope, perhaps as early as 1600. It might be hard to tell whether the construction of the telescope did occur to anybody before Lapprey, but it was certainly not made known either one way or another, and not even put into practice; and this circumstance would no more detract from the merit of Lapprey than from every other inventor before and after. Moreover, the circumstance that the inventor was obliged to work exclusively for government, at least in the commencement, might have tended to put him in the shade to the advantage of other makers. As for Galileo, it is proved beyond doubt that he had not the least part in the invention; but he knew how to use it, and enjoyed it therefore more than the inventor himself. That telescopes may not have been known to other nations in past ages cannot be positively asserted; but there is nothing to show it; and it is an historical maxim that when all the facts can be explained in an ordinary manner we are not warranted in assuming any thing out of the usual course of events.

W. DOBERCK.

Notes of an Astronomical Experiment made on the Blue Mountains, near Sydney, N.S.W.

IT has long been my intention to try what might be gained by taking a large telescope on to the mountains. Some four years since, when selecting the Transit-of-Venus stations, I made a hurried trip for the purpose of testing the fitness of the air at Woodford for observing the transit of Venus; my attention was chiefly directed to the state of the air during the day, and it proved very satisfactory. I went there in June (our winter); and although the air during the day was remarkably clear and steady, yet an hour after sunset the stars became so unsteady that no satisfactory work could be done.

My objects in making the present trip were:—first, to test the spectrum, and see if the air-lines between the D lines were the same as at Sydney; and, second, to test the air with a large telescope. There were some questions also about the thickness of our air-currents which I wished to get answers to; and, lastly, there was a desire that the colony should have some share in answering one of the most pressing astronomical questions of the day, viz., Can any thing be done by going on the hills, to improve the performance of *large telescopes*?

The results obtained by Piazzi Smyth more than 20 years since were quite enough to lead to other trials; yet little has been done in a direction which promises so much for physical astronomy.

I thought it best to go in October, between our winter and summer, when it might be expected that the radiation would not be great, and I was anxious to get some views of Jupiter before he passed away to the sunlight. Woodford, 2200 feet high, was selected, because the supports for the Transit-of-Venus instruments were ready for use; and to a large extent I was influenced by the kindness of A. Fairfax, Esq., who placed his country house at my disposal; and so made it unnecessary for me to encumber the expedition with tents to live in, and all the baggage that must go with them.

Mr. G. D. Hirst volunteered to go, and I was then sure of enthusiatic assistance.

I left Sydney on the evening of the 15th; on the morning of the 16th, at 8 A.M., the observatory (in parts, of course) and the instruments were left at Woodford by the train, and by the evening of that day, and in spite of a steady gale of wind, the observatory was up and roofed, and the heavy parts of the instrument were in their places; on the second day we were able to begin observing about noon.

The instruments consisted of:—(1) the 7¼-inch Merz telescope with equatoreal stand, clock, driving-gear, micrometer, eyepieces, &c.; (2) a 4¼-inch Cooke telescope equatoreally mounted; (3) the large spectroscope having dispersion of 18 prisms of 64°, fine micrometer, and every appliance to render it complete; (4) heliostat driven by clockwork; (5) electroscope, thermometers, &c.

Of the quality of the 7¼-in. telescope I need say nothing more now than that it is a first-class instrument, as these results will show, and well adapted for the purpose.

The spectroscope was made by Hilger, of London, and at the time (1876) it was considered the most powerful and perfect one in the world.

At 11.30 A.M. of the 17th October I began the spectroscopic work, and was at once struck by the clear definition of the lines: the gain in clearness over the Sydney atmosphere was very striking; but the next moment I asked myself, where were the lines? instead of seven lines between the two D's there was but one, and on either side of them, where it is usual to see a host of lines, the spectrum was remarkable for their absence. The line D_3*, however, which

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* [This is not the bright line of the chromosphere usually termed D_3.—ED.]

I discovered in 1877, was visible definitely, but very fine; and on very carefully examining the space between the D's there was a shading such as would be produced by very fine lines, too faint to be seen separately. These observations were made about noon, and I did not again look at the spectrum until near sunset, when a great change had come over it. Six lines were visible between the D's and the usual host on either side; they were more clearly defined than I had ever seen them before, and as the sun approached the horizon they thickened very rapidly. This will be best understood by reference to the drawings (Plate II.).

The 18th October proved a cloudy day and night, with a light easterly wind or sea-breeze. Towards the morning of the 19th, the clouds began to break, and soon after 9 A.M. the sun shone out: at 10.30 A.M. there were a few passing clouds only; but the sky was very white, as it is in Sydney, and looking at the spectrum I found six lines between the D's. The clouds were disappearing fast, and by 11 A.M. it was difficult to make out the six lines; they were fading fast; on either side there seemed to be a shading such as was seen on the 17th. At noon the air-lines had faded, and the spectrum appeared as in figure 1 (that is, without air-lines, those shown being evidently part of the solar spectrum). At 4 P.M. seven lines could be distinctly seen between the D's, and the first set of measures were taken. At 5.20 P.M., when the air-lines were getting better defined, 20 lines were measured between Ca and D_1 where only three had been seen at noon; their positions are shown in figure 3 as well as the D lines seen at the same time. After these measures had been taken, the micrometer was removed and an eye-piece that defined a little better was used; and now 24 lines could be counted between Ca and D_1, and for the first time an eighth line was seen between the D's; and just as the sun was setting, the air remaining perfectly steady, 26 lines were counted where only 20 had been measured; in fact it seemed as if new lines were coming into the spectrum every minute, and one ceased to wonder at the loss of light, when the lines, like so many strokes from a pen, came rapidly one after the other, each one definitely blotting out a part of the sunlight.

Another fact was now clearly made out: in Sydney, owing to the unsteady air when the sun is near the horizon, it had been supposed that the thickening at D_2 was all in that line, and that D_3 was lost; but now it was evident that it was D_3 which became thick and equal to D_2, so that only the faintest line of light separated the two (see figures 2 & 4).

While observing the spectrum to-day, a bright band was seen between the D's near line numbered 6. It could not have been an effect of contrast, and was distinctly seen by Mr. Hirst as well as myself: the mean of three measures gave me 5893·05 as the wave-length.

On the 20th October the spectrum observations were similar to those obtained on the 19th; but it was observed that the line 4 between the D's (that is, the one always there) seems to get wider as the sun sets without getting blacker as the others do, as if it were a double line, or that another air-line appeared alongside of it, which, with all the dispersive power of the spectroscope, I cannot separate. This has been suspected before, but now it is almost beyond question, the air is so steady and the definition splendid.

The mean of three measures taken of the lines between the D's give the following results: the whole space D_1 to D_2 is covered by 2·655 rev. of the micrometer, and the figures here given represent from line to line the spaces measured, by placing the micrometer-wires on each line in succession.

D_1. 1. 2. 3. 4. 5. 6. 7. 8. D_2. D_8.
|·336 | ·366 | ·360 | ·356 | ·203 | ·256 | ·263 | ·346 | ·131 | ·201|

In May 1877 I brought under the notice of the Royal Society in Sydney some measures made in Sydney of the lines between the D, and compared them with Dr. Huggins's drawing made with the Oxford spectroscope in 1868, showing twelve lines between the D's, and with Colonel Campbell's drawing made in London, and showing 19 lines; since then Dr. Huggins has sent me a correct copy of his drawing, which places the lines slightly different from those in my rough copy, but they are still very unlike the lines seen here. Of the quality of the spectroscope nothing more need be said, than that in London 24 lines were seen between the D's by the maker of the instrument, Mr. Hilger. It is evident, therefore, since the same instrument only shows eight lines here, that there is much less of the absorptive material here than in the atmosphere of London. Knowing the proverbially clear atmosphere of our mountains I was anxious to see if there was still less there than in Sydney; and on a fine clear day, with dry wind at noon, all the air-lines in the part examined seemed wanting; but as the Sun descended to the west the lines gradually appeared, and when he was near setting, and the vapours therefore began to condense about the hills, the lines were seen conspicuously; or if an easterly wind brought the sea air on to the mountains the lines could be seen at noon. It would seem therefore that the cause of these lines is water vapour; and if any of the 24 lines seen in London are due to gases which are the product of manufacture, we in Sydney have so far not contaminated the air in this way, for all that could be seen in Sydney are also visible on the mountains.

The definition of the spectrum lines on the mountains was a sight never to be forgotten; and it is difficult to assign a value for the gain, but it cannot be less than 50 per cent.; the lines seemed so hard and definite that there was no difficulty in measuring or in dividing close lines that under other circumstances could not have been done.

With regard to the telescopic work the gain was fully equal to that recorded with the spectroscope, and there were many exclamations from the observers about fine definition. In observing the stars during the day for adjusting the equatoreal we had abundant proof of the good quality of the air; the sharp clear disks gave conclusive proof that the air was perfectly steady. At night what seemed most striking at first was the perfect blackness of the sky: the planets seemed as if hung in space where there was nothing to reflect light but themselves; but as one looked, the perfect clearness and good definition gave us views of Jupiter such as, I think, I never saw equalled in Sydney with the large reflector of 11⅓-in. aperture; there was such an amount of detail that the eye scarcely had time to take it all in before the scene changed by the rotation of the planet. As to drawing it, that was quite out of the question. Mr. Hirst and I both tried, but the drawings only show a fraction of what we saw. It was just before and after sunset that the air seemed so perfectly steady; later in the night there was a little motion, but nothing like so much as I had seen in the winter at the same place.

I had selected some severe star tests: the companion to Rigel, one of the first found with a six-inch telescope, and only made certain by one of 18½. We were very anxious to try what a first-class 7¼ inch would do in the mountain air. With all powers over 400 the closest of known double stars was clearly elongated, and with 800, the highest power we had to use, it was in moments of best definition divided, though the distance cannot be more than ¼ of a second; with this high power Rigel presented a hard sharply defined disk, sufficient proof of the telescope and the air.

Rigel and companion were examined again on the 20 October; and Mr. Fairfax, Mr. Hirst, and myself independently assigned the same position-angle, namely 64°, and in Sydney the same angle was recorded when observing with 11⅓ inches. At the time I had not Mr. Burnham's measures to refer to, and was not a little surprised when I saw that he made it 179°.

Many other stars were examined. *α* Indi had been suspected in Sydney, but at Woodford he was unmistakably single. On the 17th a very beautiful double star not in Herschel's list was found, R.A. $21^h\ 28^m$, and Dec. S. 83° 20′, magnitudes 9 and 10, distance 2″·82, angle 112° 10′; larger, white; smaller, bright blue.

Herschel's double 5326 cannot be found; there is no such double in the place assigned. 5221 in Herschel's list: angle is given 124°; it is now only 49°·18, and distance 10″·29. Herschel's 5132: angle is 180° in error; it should be 312°. Herschel's 3419: colours not mentioned by him; as now seen, large star greenish yellow, and companion copper-red. At R.A. $18^h\ 21^m$, Dec. S. 66° 22′, I found on 13 August, 1873, a very pretty double star; large, yellow; smaller, blue; distance and angle, then 5″·02 and 295°·14; distance measured at Woodford 3″·79, and angle 288° 43′, so that there appears to be decided motion. At R.A. $1^h\ 34^m$, Dec. S. 56° 41′, a fine double; both, yellow and 7th magnitude; distance 6″·28, angle 235° 0′.

Some time was given to Saturn; but he is by no means in a good position for observation just now; the dark black line which I first saw some four years since as a faint black line at the edge of one of the belts has now become a conspicuous object even in a small telescope. The only observation worthy of note here was that the ring could be seen some distance on to the limb of the planet by its superior brightness.

On the afternoon of Oct. 20 the third satellite of Jupiter was seen in transit as a black body, small and well defined; as it neared Jupiter's limb the blackness seemed to get hazy as if it were seen through a mist; and at $7^{h}\ 27^{m}$ P.M., Sydney mean time, I lost sight of it. Four minutes later it was detected as a small bright bulge on the limb of Jupiter, which looked brighter than the surface of the planet. At $7^{h}\ 36^{m}\ 40^{s}$ egress was complete, and there was nothing about the appearance of the satellite then that would have given rise to the suspicion that it was black a few minutes before.

During the daylight hours Mr. Hirst employed a part of his time looking up Venus near the Sun, the Sun itself, and many other objects, of which very fine views were obtained; but the only observation that I would here place on record was made on the morning of the 21st October at 9 A.M., when, on looking at the Moon, he found that a large part of it was covered with a dark shade quite as dark as the shadow of the earth during an eclipse of the Moon; its outline was generally circular and it seemed to be fainter near its edges; conspicuous bright spots on the Moon could be seen through it, but it quite obliterated the view of about half of the Moon's terminator (or that part where the sunlight ends), while those parts of the terminator not in the shadow could be very distinctly seen. I should estimate the diameter of the shadow from the part we could see on the Moon as about ¾ that of the Moon.

This is one of those remarkable facts which being seen should be recorded, although no explanation can at present be offered. One could hardly resist the conviction that it was a shadow; yet it could not be the shadow of any known body, and if produced by a comet it must be one of more than ordinary density, although dark bodies have been seen crossing the Sun which were doubtless comets. No change in the position of the shade could be detected after three hours watching.

The attempts made to determine the thickness of the air-currents did not give any satisfactory information; the greatest altitude reached with a kite (1000 feet) did not get through the N.W. current.

In this short account I have omitted many things of interest, and will only now say that for some weeks before I went on the mountains the weather at Sydney had been very unfavourable for observation; and during the time I was at Woodford it seemed the same. The nights fine on the mountains were fine in Sydney; yet there was no improvement in the quality of the air; so that the great gain experienced at Woodford seems only to be accounted for by the fact that we were 2200 feet above Sydney, and therefore avoided so much of the worst part of the air nuisance.

1878, Nov. 26. H. C. RUSSELL.

Stationary Meteors.

The following stationary meteors have lately been recorded by me during some regular period of watching at Cambridge, Mass., U.S.A. As they are often of value in determining the exact radiant-centre of a meteor-shower, especially when the shower is a feeble one, particular attention should always be given to noting as accurately as possible their positions.

I find as a general thing that they are not of frequent occurrence, but it will be noticed by the list given below that an unusual number were recorded during the month of October. When accordant meteor-showers could be found in the catalogues accessible to me I have inserted the same for comparison.

I. Sept. 29th, = 1st mag., at R.A. 15°, +3°, visible one second; orange; position approximate.

II. Oct. 1st, = 4th mag., at R.A. 8°, +15°, quite accurately observed.

III. Oct. 20th, = 2nd mag., at R.A. 355°, +12½°, visible one second; position accurate. Schmidt records a shower at R.A. 347°, +14°, Oct. 18–27.

IV. Oct. 21st, = 4th mag., at R.A. 39°, −18°; position approximate. Schmidt, Oct. 19–27, at R.A. 34°, −14°.

V. Oct. 22nd, > 1st mag., at R.A. 24°, −1°, seen through haze, apparently stationary; visible one second. Schmidt, Oct., at R.A. 27°, −1°.

VI. Oct. 29th, = 2nd mag., at R.A. 31°, +13½°; position accurate. Denning, Oct. 28–Nov. 13, 1877, at R.A. 30°, +16°. Tupman, Oct. 13th, 1869, at R.A. 28°, +10°, accurate.

VII. Nov. 23rd, = 4th mag., at R.A. 54°, +9°, accurately observed. Schmidt, Dec., at R.A. 55°, +5°. Greg's Catalogue, 1876, Dec. 5–25, at R.A. 53°, +10°.

Edwin F. Sawyer.

Flammarion's Catalogue of Double Stars.

The want of a work on Double Stars, in which should be collected and discussed the observations hitherto scattered throughout various scientific publications, has been long felt. Admiral Smyth's 'Celestial Cycle' has been long out of date and out of print, and our knowledge of double stars has of late years increased so enormously that this book would not with any reasonable amount of revision serve even as a basis for the work that is so much wanted. Sir John Herschel, it would seem, after the completion of his Descriptive Catalogue of Nebulæ, had it in mind to form a grand catalogue of Double Stars, with a discussion of all the known observations of each; but he was not spared to finish his gigantic task, though he had completed the "Synoptical History" of 4000 out of the 10,000 stars in his Catalogue. After his death, the bare catalogue of approximate R.A.'s and N.P.D.'s was published in the 'Memoirs of the Royal Astronomical Society,' but without the slightest indication of the position-angles, distances, and magnitudes of the objects, and thus the work is absolutely deficient in that which most concerns the practical astronomer. The Synoptical History of the 4000 stars remains in manuscript in the hands of the Royal Astronomical Society! Under these circumstances we cannot but feel grateful to M. Flammarion for having so enthusiastically taken up this important task, to which he has devoted five years of hard work.

On attempting a discussion of the nature of double-star systems, M. Flammarion found himself stopped on the threshold by the complete absence of statistics. The questions which presented themselves as to the proportion of optical, orbital, and physically associated couples could only be answered by a detailed examination of each one of the 11,000 double stars hitherto discovered, and a discussion of more than 200,000 observations made on them. This work he resolutely undertook.

From the Introduction we learn that the present catalogue forms the second part of a work on Double Stars, which will appear shortly, and contains all the Double and Multiple Stars which appear to be certainly in relative motion. These groups number 819, of which 731 are double, 73 triple, 12 quadruple, 2 quintuple, and 1 sextuple, and in the catalogue are given some 14,000 complete measures of these objects. Amongst the couples which have been most frequently observed, M. Flammarion cites γ Virginis, of which 200 measures have been made in 160 years, since the first observation in 1718, ζ Cancri measured 300 times, Castor and ξ Ursæ Majoris each 200, and α Centauri first recognized as a double star in 1689. It is a fortunate circumstance that the principal couples were all measured by Sir W. Herschel about the year 1777, so that for a considerable number of stars we have a whole century of observations.

At the very commencement of his work M. Flammarion was stopped, in the case of a large number of couples, by the insufficiency of the observations. To remedy this he enlisted the assistance of a number of observers, chief amongst them being Messrs. Gledhill, Wilson, Seabroke, Burnham, and Dembowski, and finally completed the work himself with the aid of the 15-inch Equatoreal of the Paris Observatory, which Le Verrier placed at his disposal at the end of the year 1876. The object-glass of this instrument being defective M. Flammarion was usually obliged to stop it down to 10 or even to 8 inches. This series of measures of about 130 stars which had been neglected in recent years is given at the end of the Introduction, and is interesting as being the first which has ever been made at a French Observatory.

The arrangement of the Catalogue itself leaves nothing to be desired. It is a perfect model of clearness combined with economy of space. The latter is a very important consideration where such a mass of observations have to be presented, and M. Flammarion has only been able to meet the difficulty by the use of somewhat small type, and by printing in triple column. Thus the whole work is kept in the reasonable compass of 200 pages octavo. For each star the name is given together with the number in W. or O. Struve's Catalogues, and the class and number of W. Herschel. This is followed by the R.A. and N.P.D. for 1880 in the second line, and the magnitudes and colours of the components in the third line. After these preliminaries come the measures of position angles and distances, with the dates and the observer's initial. The observations are then carefully discussed and a summary of the history of each couple is given, the character of the motion, whether orbital, rectilinear, or merely an effect of perspective, is indicated, and the grounds given on which M. Flammarion has based his conclusion. Each of the couples has been made the subject of a geometrical diagram, which has served to decide whether the motion is curvilinear or rectilinear, and to give its direction and velocity. To complete this investigation satisfactorily, M. Flammarion found it necessary to form a new catalogue of proper motions, which is now in the press. He had also commenced the calculation of the orbits of the principal binaries by a graphical method; but Dr. Doberck's exhaustive investigations in this field (of which the results were given in the 'Observatory,' Nos. 16, 17, 18, and 19) rendered this unnecessary. M. Flammarion has therefore simply given the elements as found by Dr. Doberck.

M. Flammarion appears to have taken great pains to collect all accessible materials and to bring his work up to the most recent date. Many observations came in while the Catalogue was in the press, and those that were too late to be included in the body of the work are given at the end under the head of "Additions." Thus we may consider that M. Flammarion in this book presents all the measures that have been made up to the middle of 1878, in a form which is remarkably convenient for reference.

The Catalogue is followed by a classification of Double and Multiple Stars, which is, in fact, the pith of M. Flammarion's work. The statistics thus collected are of the highest importance as a groundwork for any physical speculations on the constitution of the universe. M. Flammarion thus describes what he has done:—"This natural classification of double stars in motion shows:—1. Systems certainly orbital, classed with reference to the observed velocity, according as they have completed, since the date of discovery, a whole revolution, three fourths of a revolution, half, a quarter, 45°, 20°, or still less; 2. Systems probably orbital, in diminishing order of probability; 3. Physical systems in which the observed relative motion is rectilinear; 4. Ternary stars; 5. Triple stars not ternary; 6. Quaternary stars; 7. Quadruple stars; 8. Optical groups; 9. Undetermined systems; 10 and 11. Maxima angular distances observed in systems certainly orbital and probably orbital; 12. Stellar systems with common proper motion in which the relative motion is rectilinear or zero; 13. Minima distances, and 14. Maxima velocities, observed in optical groups; 15. Directions of motion in orbital systems. It will be remarked, as regards the last point, that there are more systems in which the motion is retrograde, *i. e.* which turn in the opposite direction to the numeration of degrees and go from north by west to south, than there are systems in direct motion which turn in the direction east-south-west-north. There are also a great number of which the plane passes through the Sun, or which are much inclined. I have added to this classification physical couples of which the components have a common proper motion, but remain fixed relatively to each other. There are 558 systems certainly or probably orbital, 317 optical groups, 17 physical systems of which the components move in a straight line, 23 ternary systems, 32 triple systems formed of a binary system, and an optical companion, &c. &c."

From M. Flammarion's statistics it appears that the binary of shortest period is δ Equulei, though after 25 years of observation it is not yet certain whether its period is 7 or 14 years. The plane of the orbit passes through the Sun, and the semi-axes major is only 0″·4; hence the difficulty in this couple. A still more difficult object is Σ 3130 Lyræ, with a period of about 16 years and a semi-axis major of only 0″·25. Amongst the binaries which have completed one or more revolutions during the period of observation, α Centauri is that which has the largest semi-axis major, viz. 21″·80, with a period of 85 years. There are 13 stars in this group, 5 in the second (270° to 360° described), 10 in the third (180° to 270°), 15 in the fourth (90° to 180°), 21 in the fifth (45° to 90°), 92 in the sixth (20° to 45°), and 202 in the last group (less than 20°), making a total of 358 couples which M. Flammarion considers to be certainly binary. On the whole this classification represents fairly the relative rapidity of motion, though some few stars appear below their proper place from the circumstance that the discovery of their duplicity is of comparatively recent date. A notable instance of this is the remarkable star β Delphini, discovered by Mr. Burnham only 4 years ago. Its two components are of fourth and sixth mag. at a mean distance of only 0″·4, and the period appears to be about 30 years, so that this star would rank as the fourth in order of orbital velocity.

Perhaps the most remarkable result of M. Flammarion's researches is that there are certain couples physically associated for which the relative motion is rectilinear not orbital. The most interesting case is that of 61 Cygni, the two components of which are known, by researches on their parallax (*cf.* Dr. Ball's investigation, 'Observatory,' No. 17, pp. 137–140), to be at the same distance from the Sun, so that this cannot be a mere optical couple, an hypothesis which is further negatived by the large proper motion common to the two stars. But notwithstanding their evident physical association, M. Flammarion finds, from a discussion of the whole series of observations—135 in number, extending from 1753 to 1878, with a change of 80° in the position-angle—that the relative motion is along a straight line. This conclusion was first announced in 1875, and is one of great importance. M. Flammarion now gives 8 systems in which he considers that the rectilinear motion is certain, and 9 more in which it is probable; but the evidence, except in the case of 61 Cygni, is far from conclusive, the observations being insufficient and the change of position-angle small. Of the 23 ternary systems there are 5, viz. ζ Cancri, Σ 2481, ϵ Equulei, Σ 2749, and ξ Scorpii—in which the orbital motion of both companions is certain. As bearing on the probability that a given couple forms a physical system, it is interesting to note that the greatest distance observed in a known binary is 22″ for α Centauri, whilst the least distance in an optical group is 2″·6 for Σ 1516. Amongst his various classes, M. Flammarion calls attention to 14 Stellar systems (with their components more than 1′ apart) which have a common proper motion and appear to be physically associated.

As a supplement to his researches on Double Stars, M. Flammarion gives a list of 13 Double Nebulæ in which a relative movement is indicated with more or less certainty by the observations. Several of these have been made the subject of special investigations, more particularly the Trifid and the Horseshoe Nebulæ, by Prof. Holden (*cf.* 'Observatory,' No. 12, p. 406), and a useful summary of the results is given by M. Flammarion. Such delicate questions can only be settled by systematic observation, and M. Flammarion does well to call the attention of astronomers specially to these objects.

The usefulness of the work to which M. Flammarion has devoted so many years can hardly be over-estimated. Of late years the interest in double stars has somewhat flagged, though there are abundant signs on all hands that it is now reviving. Thus a large number of interesting couples had been sadly neglected, probably for want of information about them. M. Flammarion's work, by showing what has been done and pointing out what remains to be done, will at once stimulate the activity of observers and guide it into the proper channels. At the same time his analysis of the materials collected and classification of the results lend a new interest to the subject.

EDITOR.

Double Stars for March.

THE following is a list of the principal objects from R.A. $5^h\ 34^m$ to $7^h\ 33^m$.

Σ 881. R.A. $6^h\ 11^m{\cdot}4$, Dec. $+59°\ 26'$. Mag. 6·4, 7·9.

Colours, white. Variable in colour? Binary. Not measured since 1869?

	°		
1869·5	95·4	0″·88	Dembowski.
79·1	95·1	......	Gledhill.

OΣ 140. R.A. 6^{h} 19^{m}·7, Dec. +15° 36′. Mag. 7, 9·5.

Colours, A blue. Probably binary.

1867·52	119°·6	3″·04	Dembowski.

Σ 932. R.A. 6^{h} 27^{m}·5, Dec. +14° 52′. Mag. 8·2, 8·3.

Binary.

	°	″	
1876·10	332·8	2·33	Hall.
78·09	331·5	2·11	Doberck.

Σ 948=12 Lyncis. R.A. 6^{h} 35^{m}·6, Dec. +59° 34′.

Mag.: A 5·2, B 6·1, C 7·4. Colours, A B greenish white, C bluish.

A fine ternary star.

		°		
A B	1876·23	130·0	1″·4 ±	Gledhill.
	·10	130·1	......	Doberck.

A C about 305°, 8″·3.

1876·10	305°·4	......	„

SIRIUS.

R.A. 6^{h} 39^{m}·7, Dec. −16° 32′. Mag. 1, 9.

1877·25	53°·4	10″·95	Hall.

According to the elements of the last orbit computed by Auwers the angle for 1878 was 58°·4 and the distance 10″·0.

The Washington observers give recent measures of the distant small star:—

1877	114°·9	72″·09.

This is the faint object discovered by Lassell in 1865; angle abou. 120° and distance about 60″.

Σ 982. R.A. 6^{h} 47^{m}·3, Dec. +13° 19′. Mag. 5·4, 7·7.

Colours, A yellowish, B bluish. Binary. Common proper motion +0″·04 in R.A., and +0″·06 in N.P.D. Probably variable.

1876·10	163°·8	6″·37	Hall.

Σ 1037. R.A. $7^{h}\ 5^{m}\cdot 3$, Dec. +27° 26′. Mag. 7·1, 7·1.

Binary.

1876·10	311°·8	1″·26	Hall.

Σ 1110 = Castor. R.A. $7^{h}\ 26^{m}\cdot 8$, Dec. +32° 8′. Mag. 2·7, 3·7.

Binary.

		°		
A B	1877	234·6	5·53	Spörer.
	78·1	235·1	5·55	Doberck.
	79·1	233·9	5·57	Gledhill.
A C	1877	163·3	73·2	Flammarion.
	79	164·0	……	Gledhill.

The small star C, probably discovered in 1776 by C. Mayer, appears to form part of the system.

Procyon.

R.A. $7^{h}\ 33^{m}$, Dec. +5° 31′. Mag.: A 1, E 11, F 8, G 8, H 7.

		°	″	
A E	1877·17	313·8	41·0	Flammarion.
A F	·17	80·5	346·5	„
A G	·17	286·4	371·3	„
A H	·17	96·8	652·0	„

E is the old companion. F, G were first measured by Powell. G was discovered to be double by Bird in 1864, and, independently, by Burnham and Dembowski. The Washington observers have added the following (magnitudes about 10):—

	°	″
B	10	6
C	36	8·8
D	50	10

Joseph Gledhill.

Meteor Notes for March.

The interval from the 17th to 31st is available for A.M. watches and from the 11th to 26th for P.M. watches. There do not appear to be many radiants hitherto observed this month, but that is explained by the paucity of observations. We especially require A.M. watches directed eastwards. There is a good shower at about 249° +49° (between η Herculis and η Draconis), and at 283° −14°. The *Draconids* I. (263° +50°) come into play on the 11th; but Mr. Greg notes that since 1870 this shower has probably been waning. Radiants at 147° +47°, 265° +23°, 277° +25°, 223° +42°, and 192° −2° should also be looked for during this and the ensuing month, into which they are apparently continued. Zezioli noted in 1868, March 26, 8.15–9.40 P.M., "6 stars derived from Castor and Pollux in various directions; also 11 from the region of the point 136° +50½°." Fireballs should be expected on the first four nights, especially on the 2nd and 4th.

Recent Observations.—Referring to the unusual frequency of meteors on December 21, noted in last month's 'Observatory' (p. 346), Mr. V. Cornish, of Debenham, writes:—"I was using my telescope in a S.E. direction that night, and noticed the meteors were much more numerous than usual. At 7.45 one = Rigel passed from 81⅓° −2° to 79° −5°; but as I had to turn round I probably did not see the whole course. At 8.33 a meteor = α Tauri came from the direction of Taurus, and left a fine streak in Eridanus. At 8.46 two small meteors were seen with slow, short tracks. The first (4th mag.) travelled from 117° +23° to 118⅓° +23⅙°; the other (5th mag.) burst at 116⅓° +22⅙° after a path of only 15′ ±, so there may be a radiant 1° *n. p.* δ Geminorum."

On January 15, 10.57 P.M., a fine meteor > ♀ was seen at Newcastle-on-Tyne. "Path about 2° E. of β Ursæ Majoris to the Præsepe in Cancer. It left a streak like a thin line of vapour (stretched from a point above the large stars of Leo), which lingered for 2 or 3 seconds." Radiant probably in Draco, near α or θ, but it is extremely hazardous to assign the centre from a single observation.

The *Taurids* II. seen by Mr. Sawyer on Nov. 26–29 ('Observatory,' p. 339) confirm the early display of that shower, traced by the writer in 1876, on Nov. 22–27, A.M. The radiant certainly endures a month. The position at 25° +26° determined by Mr. Sawyer is identical with one of Heis's long-continued centres, namely G_1, 24° +27°, 45 meteors, Oct. 17–Nov. 30. The shower near α Aurigæ (76° +46°) was also well seen by Heis at 74° +45°, 38 meteors, Dec. 1–15.

W. F. DENNING.

CORRESPONDENCE.

To the Editor of 'The Observatory.'

The Pulkowa Double-star Observations.

SIR,—As M. Struve has honoured me by making some remarks on certain suggestions made by me in my account of Vol. IX. of the 'Observations de Poulkova;' and as one of the questions raised by him is a point of some importance, I must ask you to allow me, though I am reluctant to spend any portion of my somewhat limited leisure time in writing any thing of a controversial nature, to make a few remarks in reply. First of all M. Struve appears to have made some mistake as to the date of the construction of the double-image micrometer. I find a description of the instrument in the Introduction to the 'Greenwich Observations' for 1840, and I remark that the Astronomer Royal there states that "an eyepiece of this construction is particularly advantageous for the estimation of the distances between the double images. . . . The angles are also observed, as I believe, with great accuracy." M. Struve, when giving 1845 as the date of construction, no doubt refers to the description of the double-image micrometer contained in Vol. XV. Mem. R.A.S.; but I wish to point out that the micrometer was used at Greenwich as early as 1840, and was at that date, in the Astronomer Royal's opinion, capable of giving results of great accuracy.

As M. Struve appears to think that the superiority of the double-image micrometer is rather questionable, I beg to refer him to Vol. IV. of the 'Leyton Astronomical Observations.' In the Introduction to this volume Mr. Barclay gives, at p. xii, a table of comparative probable errors of measures of double stars made with a wire- and with a double-image-micrometer, derived from a considerable number of observations, and I must say that at all events the *primâ facie* evidence is strongly in favour of the latter. With regard to the question of 'personal equation' in double-image measures, M. Struve has misunderstood my meaning; I suppose I did not express myself clearly. By saying that double-image measures would be more free from 'personal equation' than those made with a wire micrometer, I by no means meant to imply that isolated observers, by using the former, would be relieved from the necessity of investigating their personal equations, but simply that these would be smaller and more constant, the construction and mode of observation giving less opportunity for the display of personal bias. The double-image micrometer possesses in this respect, I conceive, the same advantage over the wire micrometer that the method of observing transits by galvanic registration has over the "eye and ear" method of observing. I am unable to admit the validity of the process by which M. Struve determines the constant error of Prof. Kaiser's measured distances, as he assumes that he has satisfactorily determined his own systematic errors from measures of artificial stars; but it is unnecessary to remind the readers of 'The Observatory' that strong reasons have been given for doubting the accuracy of this method; it is sufficient to mention, in connexion with this subject, the name of the illustrious Bessel*. And I do not think that astronomers generally are prepared to accept the results of the artificial star-measures unless they are confirmed by some independent investigation.

As it is I can only admit that M. Struve has shown that his measures of distances with a wire-micrometer differ by a certain quantity from those made by Prof. Kaiser with a double-image micrometer, which evidently throws no light on the subject of absolute personal equation. Is not M. Struve here guilty of the fallacy called by logicians *ignoratio elenchi?*

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* For Bessel's criticism see his 'Astronomische Untersuchungen,' i. p. 306.

I come now to the most important question raised in M. Struve's letter, viz. the accuracy of the mean of measures made by several observers. I conceive that it must be admitted *à priori*, and that the tendency of modern astronomy is to admit more and more, that the mean of results obtained by several observers is the most accurate that can be given, assuming that the systematic errors peculiar to each are thus eliminated. This principle underlies the system of combining the results of meridian transit observations which prevails at Greenwich, and I believe at all other observatories where independent catalogues are formed: this principle underlies the method of forming standard places of stars, *i. e.* places free from systematic errors, such as Newcomb's Right Ascensions and Auwers' Declinations; and I hold that the same principle should be applied to measures both of position-angles and distances of double stars. I do not consider the fact that M. Struve finds that the means of the measures of position-angles made by several observers agree with those deduced from his artificial stars as any proof of the principle in question; but, as far as it goes, it is a presumption in favour of the accuracy of M. Struve's method of deducing the systematic errors of his measures of position-angles, and I merely referred to it as a circumstance by which his attention might have been called to the principle. With regard to the distances as measured by several observers, it seems to me that the fact of their all appearing too small as compared with the results of the artificial star-measures indicates that there is something wrong with the latter, and does not militate against the soundness of what I may call the modern doctrine.

M. Struve is, of course, the person to decide whether six of the Pulkowa observers could be employed on the duty of measuring double stars, but I should have thought that the Imperial Observatory of Russia could have supplied this number for the promotion of such a valuable work. I must, however, remind M. Struve that accuracy of results, and not an accumulation of measures, is the great aim of modern astronomy, and that no amount of available power is wasted in the attainment of this object. Whether, I may ask, is more valuable: a catalogue of 1000 stars each observed only once, or one containing 200 stars, each observed 5 times? I have no doubt that a more real increase of knowledge is gained by having 1000 measures of double stars made by six observers (if in different localities so much the better), than if a single observer were to make 6000 measures, besides observations of artificial stars. in the words (slightly altered) of the Poet-Laureate—

"My faith is large in MEANS."

I am, Sir, yours faithfully,
A. W. DOWNING.

Greenwich,
1879, Feb. 10.

The Sun's Corona and Meteor-Streams.

Sir,—

I am obliged to Dr. Schuster for his remarks on this subject in your last number. I can comprehend his evident disinclination to be considered as in any way a champion of the meteoric theory of Sun-spots. If, as I imagine, Dr. Schuster's letter gives on the whole a negative answer to the question I asked at the end of my note in No. 21, still I must say I do not think we have any sufficient evidence to show that meteoric matter was so extraordinarily rare in the corona at the time of the last total eclipse as the almost total absence of Sun-spots in 1878 would seem to indicate; and surely this remarkable absence of meteoric particles might have been expected to announce itself to the observer in some more unmistakable manner than by a not very definite supposed change of symmetry in the shape of the corona!

As to the question of the brightness of the continuous spectrum, I should like to quote a passage from an interesting article "On the Recent Solar Eclipse," by Prof. C. A. Young, reprinted from the 'Princeton Review.' He says:—"It may be a question whether the continuous spectrum was or was not more brilliant than usual. Mr. Lockyer thinks it was, and explains it by the hypothesis that the absence of incandescent gases thrown up from the solar nucleus renders cooler than ordinary the regions surrounding the Sun, and thus permits an unusual quantity of the meteoric matter, which is supposed to be always abundant in that neighbourhood, to remain unvolatilized, and so capable of reflecting sunlight." This is the only reference I have seen as to the relative brightness of the continuous spectrum in the 1878 eclipse and in former eclipses.

I remain, Sir, yours faithfully,

James L. M^cCance.

Rathfern, Rayner's Road,
Putney Hill, S.W.
1879, Feb. 17.

NOTES.

Researches on the Motion of the Moon[*].—For some years after the appearance of Hansen's 'Tables de la Lune,' it was generally believed that the lunar theory had been perfected at last, and it was supposed that the new tables represented with great exactness the motion of the Moon in the past and would long enable its place to be accurately predicted for the future. Unfortunately this was not true; these anticipations have not been realized, and less than twenty years have sufficed to show that we have not yet entirely mastered the intricate theory of the motion of our troublesome satellite. As early as the year 1870 (American Journal of Science,

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[*] "Researches on the Motion of the Moon," by Prof. Simon Newcomb, App. II. to the 'Washington Observations' for 1875. Washington, 1878.

June 1870) Prof. Newcomb pointed out that although the tables seemed to give with accuracy the place of the Moon for the century between 1750 and 1850 yet they appeared to entirely fail to represent the observations made prior to this epoch. It is true that there did not seem to exist any readily available means of determining the true place of the Moon at this early date, and it was commonly supposed that our exact knowledge of the true place of the Moon did not extend further back than Bradley's observations, commencing in 1750. Such observations as were available, however, were made use of by Prof. Newcomb, with the result of indicating the existence of grave discrepancies between the observed and tabular places of the Moon. This was a very grave conclusion, for it justified apprehension as to how long the new tables would continue to accurately represent the motion of the Moon. But what rendered it still more serious was the ominous fact that the most recent observations showed that the observed place of the Moon was steadily deviating from the place assigned by the tables, and grave fears were entertained that it would before long become impracticable to accurately predict the place of the Moon from these tables of Hansen's. It seemed to be certain that there must be some grave defect in the theoretical basis of Hansen's tables, and especially in his investigation of the terms of long period in the mean longitude of the Moon.

At the earnest desire of Prof. Newcomb, the work of reinvestigating this subject, and, if possible, of ascertaining the cause of these deviations, was made part of his official duty as Professor to the United States' navy. The work was divided by him into two portions:—

"I. The mathematical theory of the inequalities of long period in the Moon's mean motion. As the only cause to which such inequalities could be attributed was the action of the planets, this part of the investigation resolved itself into a computation of that action.

"II. The study of the inequalities in question from observations, especially from observations prior to 1750. In the ancient and modern observations of eclipses and occultations, there was believed to be an immense mass of valuable material for the purpose in question, some of which had been almost forgotten, and very little of which had been discussed with modern data."

Unforeseen difficulties were encountered in the mathematical investigation of the theory of the terms of long period, so that this part of the work still remains unfinished, and it is owing to this reason that the second portion of the work now makes its appearance as the first part.

For the purpose of properly investigating the theory of the terms of long period it is most important that the theorist should be able to compare the results of his mathematical investigation with a long series of observations of the Moon. Hitherto he has had a series extending back only as far as 1750—that is to say, barely a century and a quarter. To be able to extend his comparisons between theory and observations for a still greater interval of time would be of the greatest importance to him. Hitherto, though there has been known to exist a considerable number of observations which ought to be capable of affording the means of deducing the true place of the Moon during the later part of the seventeenth and early part of the eighteenth century, yet these observations have never been properly reduced and systematically compared with good lunar tables. Prof. Newcomb has now removed this difficulty, and with great care reduced all these observations, and deduced from them the true place of the Moon, which he has compared with Hansen's tables and so ascertained the discrepancy between the observed and tabular places of the Moon. Not content with this, Prof. Newcomb has been successful in disinterring a great number of most valuable observations made by the early French astronomers, Picard, Cassini, Maraldi, and Delisle. These observations had been almost completely forgotten, and astronomers are greatly indebted to Prof. Newcomb for his discovery of their existence, buried, as they were, in the Archives of the Paris and Pulkowa observatories, and still more for his successful decipherment of their meaning and for their skilful reduction and discussion.

Through these observations, Prof. Newcomb has been able to determine with considerable accuracy the place of the Moon between 1670 and 1730, and with inferior accuracy between 1625 and 1670. He has thus doubled the interval of time through which astronomers by their observation can trace with some exactness the path of the Moon; and when the theorist wishes to compare the result of his mathematical analysis with the result of observation, instead of being limited to the century and a quarter between 1750 and 1875, he will be able to make use of the two and a half centuries from 1625 to the present time.

Many of the observations recorded in this volume, with all the data for their reduction, as well as the reduced observation, will be found to present features of great interest to the astronomer, and throw much important light on the methods of observation employed by the early astronomers. Much light is also thrown on the inner life at the Paris Observatory; and this goes far to explain its comparative failure.

Besides the more modern observations, Prof. Newcomb has reduced the eclipses of the Moon quoted by Ptolemy in his 'Almagest' and a numerous series of observations of the eclipses of the Sun and Moon observed by the Arabians in the middle ages. Throughout, all the observations are compared with Hansen's tables of the Moon.

The results which Prof. Newcomb has derived from his comparison of the observed and tabular places of the Moon, and which are given in full in this volume, have been already published in the 'American Journal of Science,' 1877 Nov., and are described in a note in the 'Observatory' for July 1878. They need not be referred to again. E. N.

PREDICTED TRANSIT OF VULCAN *.—From a discussion of eight records of round black spots seen on the Sun, which may possibly have been an intra-Mercurial planet in transit, M. Oppolzer has deduced elements which, though differing greatly from those found by Le Verrier, satisfy fairly all the observations. But on further consideration M. Oppolzer is led to doubt the certainty of these elements, seeing that with such a small inclination (7°) and periodic time (15·8 days) as he finds, transits must occur every year in March and October, and it is therefore difficult to explain the circumstance that they have so seldom been observed. And he remarks that by sufficiently diminishing the periodic time it is possible in every case to find a satisfactory solution, so that the tolerable representation of eight observations may after all be nothing but an accidental coincidence. However this may be, there is an easy means of deciding the question, as a transit of M. Oppolzer's Vulcan ought to take place on March 18 next, as follows:—

Ingress	$17^h\ 14^m$	G.M.T.	Pos.-Angle	74°.
Egress	22 41	„	„	254.

Observers should keep a watch on the Sun on the morning of March 18; and if nothing is then seen of Vulcan, we may infer that M. Oppolzer's elements are not correct. It is to be remarked that five of the observations in M. Oppolzer's list were used by Le Verrier for his solution. In the case of the other three observations, there is no mention of any motion of the spot, and two of them were expressly rejected by Le Verrier on this ground. M. Oppolzer's elements do not represent either of the objects noted by Prof. Watson in the late eclipse.

THE SUN-SPOT CYCLE.—In a recent communication to the Metropolitan Scientific Association, Mr. A. P. Holden summarizes the various results which have been supposed to show a certain connexion between sun-spots and other phenomena. After alluding to the influence which, according to the investigations of Messrs. De La Rue and Balfour Stewart, is exercised by the configurations of the planets on the spots, and the parallelism which appears to exist between these latter and the diurnal variations of the Earth's magnetism and the aurora, Mr. Holden discusses the question whether the corona waxes and wanes with sun-spots. He points out that Prof. Young, Mr. Lockyer, and other experienced observers describe the corona as much fainter in 1878 than in 1869, 1870, or 1871, though nearly all agree in giving it a much greater extension. There seems to have been a confusion in the minds of some persons of extent with brightness, though the very fact of the great extension of the corona is a necessary result of a limitation of its brightest portion, which in previous eclipses has by its intense brilliancy drowned out the fainter outlines. The photographs taken

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* Astronomische Nachrichten.

in 1870 show a greater extent of corona than any in 1878, whilst in 1871 the pictures show nearly a like extent, though the exposure was extremely short as compared with those of 1878. Further, whilst in 1871 Respighi traced the 1474 zone to a height of 6′ or 7′, and Lockyer saw a ring formed by the same line some 2′ high, the last-named observer with similar instruments could not see the 1474 line at all during the late eclipse, thus showing a marked diminution in the brightness of the gaseous corona. Mr. Holden further quotes Prof. Young's emphatic testimony to the same effect, as against the views which have been put forward by Mr. Ranyard *. As regards the possible connexion between Sun-spots and our weather, Mr. Holden thinks we need data very different from what are at present used in such attempts, and that, in fact, we require to know the meteorological history of the year in all parts of the globe. This latter is, he considers, supplied by the phenomenon of the aurora, which Prof. Loomis has shown to vary with the sun-spots. In this view he adopts Mr. F. Pratt's hypothesis that the aurora is simply light filmy cirrus cloud, first deposited at the base of a vast upper body of highly rarefied vapour, and illuminated by the free electricity escaping in the condensation through the very rarefied medium above, towards the north or south. The aurora would, according to this theory, have its origin in a vast electrical storm, resulting from a violent condensation of vapour which causes a flow of electricity from the pole to restore equilibrium. The aurora would thus depend on storm phenomena of an intense character, and the frequency of aurora at the Sun-spot maxima would indicate the connexion of the latter with the weather.

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* In a remarkable letter to the 'Athenæum' on this subject Mr. Ranyard makes the extraordinary statement that "the rings seen by Mr. Lockyer in 1871 corresponded to the area of the chromosphere and not of the corona." With reference to this assertion it is sufficient to remark that the chromosphere is only 10″ high, and that at the time of Mr. Lockyer's observation it was completely hidden by the Moon, being some 51″ below the Moon's limb on the one side and 26″ on the other. How it could, under these circumstances, be seen as a ring some 2′ high, Mr. Ranyard does not explain. In the same letter Mr. Ranyard quotes the fact that no bright lines were seen in the coronal spectrum during the eclipses of 1842 and 1868, without appearing to be aware that these observations were made near a time of minimum of Sun-spots, and that their agreement with the results of the late eclipse is, *pro tanto*, an argument that the gaseous part of the corona *does* vary with sun-spots, the hypothesis which Mr. Ranyard sets out to upset. We can hardly congratulate Mr. Ranyard on the knowledge of the subject which he appears to have acquired by his prolonged study of the long-delayed "Eclipse Volume."

THE CONSTITUTION OF THE SUN*.—In the French 'Annuaire' for 1879, M. Janssen gives, under the title "Recent Progress in Solar Physics" an account of his own discoveries in this field, with occasional allusions to the want of success of other physicists. Passing over the observations of the eclipses of 1868, 1869, and 1870, we may specially note, in the Indian eclipse of 1871, M. Janssen's observation of the dark D lines as well as some others in the green, in the spectrum of the corona at a height of 3′ to 6′, thus showing the presence of reflected sunlight in the corona, though the faintness of these dark lines, notwithstanding the light-grasping power of Janssen's instruments, indicated that this reflected light was drowned by the light emitted from the corona itself. M. Janssen further records that the polarization of the corona was radial, with a maximum at a distance of some minutes from the limb, a circumstance which he explains as a consequence of the strong emission in the lower regions which masks the reflected light. As regards the constitution of the corona, M. Janssen is inclined to adopt the theory that it is composed partly of a gaseous envelope or atmosphere and partly of meteor-streams, which give rise to the appearance of gigantic petals, as of an enormous dahlia. He agrees with Prof. Young and others in holding that the corona will vary with the solar prominences, and consequently with the sun-spots, and points out that in 1842, at an epoch of minimum, its outline was regular and concentric with the Sun, whilst at the maximum it would be broken up by jets from the chromosphere. The true character of the phenomenon may, however, be partially masked by the meteor-streams, which complicate the appearances.

The latter part of M. Janssen's paper is devoted to an account of his photographs of the solar granulations, to which we have several times alluded ('Observatory,' No. 4, p. 114; No. 8, p. 255; No. 9, p. 265; No. 12, p. 410). Two beautiful photographs, taken at an interval of 50 minutes (reproduced by the "Photoglyptic" process), show in a striking manner the remarkable changes which are continually taking place in the solar photosphere.

Besides M. Janssen's article, the 'Annuaire' contains, as usual, a great amount of astronomical and general information, including ephemerides of variable stars, elements of all known planets (major and minor), satellites and periodical comets, meteor-radiants, tables of weights, measures, &c., latitudes and longitudes of nearly 300 places in various parts of the globe, magnetic declinations, together with tables of density, expansion, refractive indices, and other physical data for a large number of substances.

HARVARD COLLEGE OBSERVATORY.—Prof. Pickering's Report for the year ending 1878, Oct. 31, has just been circulated. Both the large telescopes have been kept actively in use. The 15-inch equatoreal has been used on 239 nights on photometric work, 18000 observations having been made with it. The light of all the known satellites, except the two inner satellites of Uranus, and of the components of all known double stars of the fifth magnitude or brighter, has been measured, various parts of the spectra of β Cygni and γ Andromedæ have been photometrically observed, and the progressive increase in the relative light of their blue components towards the more refrangible end of the spectrum has been clearly shown, and the phases of twenty-three eclipses of Jupiter's satellites have been observed by help of a photometer, an average of nine determinations of brightness of the satellite as it enters or emerges from the shadow having been made for each eclipse. For these various kinds of work a number of photometers have been designed, lettered from A to O.

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* Annuaire pour l'an 1879, publié par le Bureau des Longitudes (Gauthier-Villars, Paris. Prix 1 fr. 50 c.)

With the meridian-circle the observations of the zone $+50°$ to $+55°$ have been continued and will be completed by the end of 1878, only 134 stars between 23^h and 5^h R.A. remaining to be observed at the date of the Report. The whole number of observations during the year is 4300. The reduction and publication of the past observations have been pushed on, and the set of Annals is now complete from Volume I to X. The amount of material still to be published will fill nine or ten volumes more; but the means for preparing it for the press, as well as for keeping both instruments in active work, are required. To meet this want a subscription has been started to raise 5000 dollars a year for five years; and of this, 3200 dollars a year have already been promised, so that it is confidently expected that in a short time the entire amount will be received.

The distribution of time-signals has been extended, and in particular a time-ball has been established at Boston. This ball is of copper, four feet in diameter, and weighs about 250 lbs. It has a fall of 15 feet and has been dropped with great regularity at noon since July 12.

DUDLEY OBSERVATORY, ALBANY.—During the past year various improvements have been made in the instrumental equipment. An excellent chronograph has been made by Alvan Clark, the object-glass of the transit-circle has been refigured, and the 13-inch equatoreal has been put in fair working order. In his Report for 1878, Prof. Lewis Boss announces that the observations of the zone $+1°$ to $+5°$ were commenced last September, and since that date the transit-circle has been in constant use on clear nights. A subscription has been started to defray the expenses of this work (estimated at 6000 dollars), and more than half the amount required has been already promised. With the equatoreal a few miscellaneous observations have been made, including 25 nebulæ noted during an unsuccessful search for Tempel's second periodical comet 1873, II. Four of these nebulæ could not be found in any of the Catalogues in the Observatory library. The Transit of Mercury was also observed (*cf.* 'Observatory,' No. 20, p. 274) and the eclipse of the Sun at Las Animas with a four-inch comet-seeker.

Prof. Boss announces that his work on standard declinations and systematic corrections is now printed, and this important discussion of all available catalogues will, we hope, shortly be distributed amongst astronomers.

This valuable investigation is to form Appendix H to the Report of the Northern Boundary Commission.

Mr. Gill has been appointed to succeed Mr. Stone as Astronomer at the Cape of Good Hope.

Ephemeris of the two Outer Satellites of Uranus.

8h G. M. T. 1879.	Titania. Position. °	Titania. Distance. ″	Oberon. Position. °	Oberon. Distance. ″
Mar. 15	185·3	31·1	185·0	41·3
16	160·1	15·0	173·1	28·3
17	47·1	14·4	133·1	13·3
18	20·4	30·7	49·6	18·3
19	9·3	33·9	26·6	33·9
20	353·4	21·6	17·3	44·2
21	262·2	9·3	10·4	45·9
22	205·9	25·9	2·4	38·4
23	193·0	34·6	347·0	24·1
24	180·7	27·3	287·4	11·8
25	135·2	10·2	220·6	22·6
26	34·7	19·8	203·7	37·2
27	16·8	33·2	195·3	45·4
28	5·7	31·6	191·3	44·6
29	342·4	16·2	179·2	35·0
30	231·8	13·4	158·4	19·8
31	201·3	29·8	81·2	26·8
Apr. 1	189·8	34·0	34·4	12·7
10	186·3	31·9	191·5	45·7
11	164·5	17·2	183·8	40·1
12	57·0	12·4	170·5	27·0
13	22·4	28·8	126·0	13·1
14	10·3	33·9	48·1	19·3
15	355·5	33·4	26·2	34·3

A. M.

Ephemeris of Brorsen's Comet (by Prof. Schulze) (during absence of Moonlight).

Berlin Noon.	R.A. h	R.A. m	R.A. s	Dec. °	Dec. ′	Log r.	Log Δ.
Mar. 15	1	44	36	3	50·5 N	9·8210	0·0975
17		51	23	5	43·0	9·8098	0·0870
19		58	15	7	39·8	9·7996	0·0760
21	2	5	11	9	41·0	9·7907	0·0648
23		12	11	11	46·7	9·7832	0·0532
Apr. 8	3	10	55	30	49·1	9·7898	9·9538
10		19	2	33	24·9	9·7986	9·9415
12		27	32	36	2·5	9·8086	9·9295
14		36	32	38	41·3	9·8196	9·9179
16		46	11	41	20·9	9·8315	9·9067
18		56	37	44	0·5	9·8439	9·8961
20	4	8	4	46	39·3	9·8568	9·8861

Berlin Noon.	R.A.			Dec.		Log *r*.	Log Δ.
	h	m	s	°	′		
Apr. 22	4	20	44	49	16·4 N	9·8700	9·8768
24		34	54	51	50·4	9·8833	9·8682
26		50	54	54	19·5	9·8967	9·8605
28	5	9	3	56	41·7	9·9100	9·8536
30		29	43	58	54·3	9·9232	9·8478

This periodicol comet was picked up on Jan. 14 by M. Tempel close to the horizon ('Observatory,' No. 22). It has since greatly increased in brightness, being six times as bright on Mar. 15, and twelve times as bright throughout April. It passes its perihelion on Mar. 30.

Astronomical Memoranda, 1879, *March* 15–*April* 30.

Sun. March 16, sets 6^h 3^m, rises 18^h 12^m; April 30, sets 7^h 18^m, rises 16^h 34^m.

Equation of time:—Sun *after* clock, March 16, 8^m 51^s; March 26, 5^m 51^s; Apr. 5, 2^m 49^s; Apr. 15, 0^m 5^s; *before*, Apr. 25, 2^m 4^s.

Sidereal Time at Mean Noon:—March 16, 23^h $34^m{\cdot}8$; March 26, 0^h $14^m{\cdot}2$; Apr. 5, 0^h $53^m{\cdot}7$; Apr. 15, 1^h $33^m{\cdot}1$; Apr. 25, 2^h $12^m{\cdot}5$.

Moon. New, March 22, 9^h; First Quarter, March 30, 13^h; Full, Apr. 6, $10\frac{1}{2}^h$; Last Quarter, Apr. 13, 2^h; New, Apr. 21, 2^h; First Quarter, Apr. 29, 2^h.

Occultations:—March 30, 7^h 16^m B. A. C. 2154 Oc. D. 14°; Apr. 1, 9^h 37^m θ Cancri Oc. D. 87°; Apr. 3, 11^h 46^m B. A. C. 3529 Oc. D. 20°, 12^h 56^m 43 Leonis Oc. D. 130°; Apr. 4, 11^h 50^m p^6 Leonis Oc. D. 27°, 12^h 49^m Oc. R. 265°; Apr. 12, 16^h 29^m χ^3 Sagittarii Oc. R. 264°; Apr. 23, 9^h 29^m 23 Tauri Oc. D. 171°; Apr. 30, 9^h 56^m B. A. C. 3398 Oc. D. 358°. The angles at disappearance (D.) and reappearance (R.) are reckoned from the *apparent* N. point towards the right hand round the Moon's circumference as seen in an inverting telescope.

Mercury is at greatest elongation E. (18° 47′) on March 29, when he sets in the N.W. 1^h 52^m after the Sun and is very favourably situated for observation. He is in inferior conjunction on Apr. 17, after which he is a morning star.

Venus is an evening star, setting at 8^h 18^m on March 16 and at 10^h 39^m on Apr. 30. Her disk is gibbous, the illuminated portion being 0·916 on March 15 and 0·848 on Apr. 15, and the corresponding diameters $11''{\cdot}0$ and $12''{\cdot}2$.

Mars is a morning star in the S.E., advancing through Capricornus.

Mar. 16, R.A. 20^h $2^m{\cdot}9$, Dec. 21° 22′ S., tr. 20^h 27^m, rises 16^h 20^m
Apr. 30, 22 16 ·1, 12 29 S., 19 43 , 14 41

Jupiter is a morning star in Aquarius.

Mar. 16, R.A. 22^h $1^m{\cdot}6$, Dec. 12° 55′ S., tr. 22^h 24^m, rises 17^h 26^m
Apr. 30, 22 36 ·2, 9 57 S., 20 1 , 14 47

Saturn is in conjunction with the Sun on Mar. 26.

Uranus is an evening star, moving slowly *n. p.*, and may be picked up about $\frac{1}{3}$ of the way from Regulus to ρ Leonis. EDITOR.

Publications received:—R. L. J. Ellery, *Observations of Mars and Comparison Stars at Melbourne Observatory*, 1877.—*Annuaire pour l'an* 1879, *publié par le Bureau des Longitudes* (Paris, Gauthier-Villars)—Prof. Piazzi Smyth, *End-on Illumination in private Spectroscopy*, 1879—E. Hartwig, *Bestimmungen der relativen Oerter einiger Sterne in der Vulpecula*—Ch. Fiévez, *Bibliographie des Ouvrages, Mémoires et Notices de Spectroscopie* (Bruxelles, F. Hayez, 1878)—Dr. H. R. Rogers, *New Theories of the great Physical Forces*—Prof. E. C. Pickering, *Annual Report, Harvard College Observatory*, 1878—Prof Lewis Boss, *Annual Report, Dudley Observatory*, 1878—*Memorie degli Spettroscopisti Italiani*, 1878, Nov. and Dec.—*Popular Science Monthly*, 1879, Jan. and Feb. (Appleton, New York)—*American Journal of Science*, 1879, Jan. and Feb. —*Science News*, Nos. 1 and 2 (Cassino, Salem, Mass., $2 per annum)— *Kansas City Review*, 1879, Jan.—*The Analyst*, 1879, Jan.—*Sirius*, 1879, Feb. (Leipzig, Karl Scholtze). EDITOR.

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THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 24. APRIL 1. 1879.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1879, March 14.

LORD LINDSAY, M.P., F.R.S., &c., *President*, in the Chair.

Secretaries: Mr. J. W. L. GLAISHER, F.R.S., and
Mr. A. COWPER RANYARD.

MR. GLAISHER reported that since the last Meeting 91 presents had been received; and the thanks of the Society were voted to the respective donors.

Mr. Knobel read a paper on a Persian MS. of Ulugh Beigh's Catalogue of Stars belonging to the Royal Astronomical Society. The MS. is in two volumes, one containing Ulugh Beigh's Introduction to the Knowledge of the Stars, and the other being devoted to the Astronomical Tables and the Catalogue of Stars. Our knowledge of this catalogue is derived entirely from a translation of some Oxford MSS. made by Thomas Hyde and published in 1665. The author has carefully translated the catalogue of stars as given in the R. A. S. MS., and on comparing it with Hyde, he has found over one hundred and twenty differences in the longitudes and latitudes of stars. He remarked that 26 per cent. of these discrepancies were due to confusion between the Arabic letters *Yā* (10) and *Nūn* (50) when in combination with other letters. In the comparison made by Professor Schjellerup between the Copenhagen and St. Petersburg MSS. of Al-Sûfi, 20 per cent. of the discrepancies he found were due to precisely the same uncertainty about these two characters. Some of the discrepancies in the old records of eclipses by Ibn Junis, which Prof. Newcomb has recently discussed in his 'Researches on the Motion of the Moon,' were investigated under the light thrown upon such mistakes by an examination of the R. A. S. MS., and probable explanations were suggested in several cases.

He remarked that the magnitudes of Ulugh Beigh's Catalogue had never been properly translated. Ulugh Beigh gave the brightness of stars in thirds of a magnitude; but unfortunately in Baily's edition they were given only in whole numbers. Against some magnitudes was the initial letter of the word "*Kabīr*," signifying "large," and against others the initial of "*Saghīr*," signifying small; so that 3 K would mean Argelander's magnitude 3–2, and 3 S in the same notation would be 3–4. The paper contained a complete list of Ulugh Beigh's magnitudes as found in the R. A. S. MS. compared with Hyde's results from the Oxford MSS.

The President, in moving a vote of thanks to Mr. Knobel for his interesting paper, suggested that it would be of great importance to our knowledge of ancient astronomy if any Fellow of the Society could undertake the collation of all the MSS. of Ptolemy's Catalogue, perhaps 50 at the outside, giving the various readings in those MSS.

Mr. Gill read a paper on a new Method of determining Astronomical Refraction. He remarked that the law of refraction was by no means known with certainty, though Bessel and Nyrén have arrived at very similar results by different hypotheses, and the determination of refraction and latitude are mixed up together. He proposed as a new method to observe at a station on the Equator with an altazimuth, pairs of stars at equal altitudes east and west of the meridian; and in this way, the true difference of R.A. being known, to determine twice the refraction at all zenith-distances. At extreme zenith-distances there would be difficulties, because the stars are spread out into spectra. The method could of course be applied at observatories not situated on the Equator. At the end of the paper he had summarized the conditions necessary for the elimination of systematic error. His attention had been particularly drawn to this subject because of the difficulties which have been found in the determination of refraction. It will be in the recollection of the Society that a very able paper was written by Mr. Stone upon this subject. So delicate and difficult is the inquiry, and so involved is it with flexure and division error, and R—D correction, and the way in which the absolute latitude of a place is mixed up with the law of refraction, that the difference of half a second in the latitude of the Greenwich Observatory is a matter not yet fully settled. The difficulty that is found at 52° will be immensely greater at lower latitudes.

Mr. Gill also read a "Note on Systematic Errors in R.A. depending on Magnitude." This was a question in the solution of which most amateurs could assist. He had found, on comparing the results of heliometer determinations with those of meridian observations, that all the R.A.'s of faint stars observed with the chronograph are too great as compared with those of bright stars; but the results for R.A. by the eye-and-ear method coincide precisely with those with the heliometer, which he believed to be free from systematic error, having subjected them to several tests. He explained this systematic error in chronographic transits as arising from the circumstance that the observer usually trains himself to press the chronograph-key so that the tap shall coincide with the passage of the star across the wire, and that a bright star, with its rings and appendages, somehow appears to be nearer the wire than a faint star. A fact had been brought to his notice by Dr. Copeland and Dr. Hartree which all amateurs could verify, viz. that in looking at a bright star and a faint one near it at a position-angle of 0° or 180°, if the telescope be made to move backwards and forwards, the bright star will appear to swing relatively to the little star, thus showing that the light of the bright star is more quickly perceived by the brain. This matter of personality would be tested by observing the transit of a bright star over part of the wires with the full aperture, and over the remainder with a wire screen applied to diminish the light.

The President. The matter which Mr. Gill has brought before us with reference to astronomical refraction is one of great importance and great interest; and I am glad, as he has brought it forward, that he will soon have the means of utilizing it in a very excellent manner. It has not yet been officially notified to the Society, but since our last Meeting the Government has appointed Mr. Gill to be the Astronomer Royal at the Cape of Good Hope; and I feel that good work will ensue from his appointment.

Mr. Marth. We may all reckon upon Mr. Gill doing his best at the Cape of Good Hope; but perhaps he will excuse my making a few practical suggestions which may be of some little service to him. The determination of refraction by means of meridian observation is really not so difficult as it would appear from the discrepancies which have been found in consequence of the neglect of some simple precautions.

[Mr. Marth then proceeded to give at great length, and amidst signs of impatience from the Meeting, the history of the subject taken from the Introduction to Bessel's 'Tabulæ Regiomontanæ.']

As allusion has been made to Mr. Stone's investigation, I must say something about it. Some twenty years ago, when four volumes of the Transit-Circle observations were published, I tried to investigate what result the Greenwich observations gave for refraction; and the first question which I had to consider was the state of the Greenwich meteorological instruments: but I could not find in the Greenwich volumes any indication of the error either of the barometer or of the thermometer. I sent a paper to the Society in which I expressed myself cautiously, mentioning what I had found about the barometer; and as regards the thermometer, that in several cases its reading was higher than that of the maximum thermometer on the same day in the Meteorological Observatory. I assumed that that might be due to a real difference of temperature; I did not assume the possibility that there could be any serious error in the Greenwich thermometer.

The President. As the matter before the Meeting is the new method of investigation pursued by Mr. Gill, I do not think it would be well to go into the history of the Greenwich thermometer over twenty years ago.

Mr. Marth. I shall merely suggest that Mr. Gill should take instruments, the errors of which are fully determined, and not alter the instruments when he finds that they have some errors, but apply the errors to his Table of refraction.

Mr. Christie. As to the meteorological instruments used for astronomical observation, there is no necessity to give Mr. Gill any advice in the matter. It is the practice at Greenwich to verify such instruments with all care; and if Mr. Marth will take the trouble to look into the Greenwich volumes for many years past, he will find that the corrections for barometer and thermometer are very carefully stated, and that the instruments have been compared with the meteorological standards. The error of the thermometer at no part of its scale exceeds one tenth of a degree; and I do not think that any one can profess to read a meteorological instrument with greater accuracy than that.

Mr. Marth. Mr. Stone's determination refers to the years 1857 to 1865, and from the year 1857 up to the middle of 1858* the thermometer used had read 0°·8 more than afterwards. Mr. Stone unfortunately has taken no notice of that.

Mr. Neison. Mr. Gill said there was one point in which amateur astronomers might assist, namely, in determining the effect of difference in magnitudes. He gave a method of doing it which would not be very easily applied by most observers. Perhaps I might mention a rather different mode of doing this?

Suppose two wires of a transit-instrument to be separated by some interval either known or not. Let a star transit over the first wire with the full aperture; from its size, it will appear to transit before its right time. Then reduce the aperture and let the star transit across the second wire. The interval between the two transits will be larger than the true interval by the amount that the brighter object appears to transit before the fainter object. Now reverse the conditions, diminish the aperture, and let the star cross the first wire, and then restore the full aperture and let it transit the second wire. Now the interval will be less than the true interval by the amount by which the brighter object transits before the fainter. It is obvious, therefore, that the difference between these values will be twice the amount by which the brighter object transits before the fainter object.

In this manner it would be easy for any amateur to throw a good deal of light on this interesting point in practical astronomy by

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[* Mr. Marth was understood to say "1868;" but this was obviously a slip. The effect of the correction (which is −0°·6, not −0°·8, in the thermometer) for 1½ years out of 9 would be quite insignificant, and does not affect to any appreciable extent the validity of Mr. Stone's result. Mr. Stone found that Bessel's refractions required to be diminished by ·00531 × refraction; this quantity would have become ·00555 × refraction if the corrections to the barometer and thermometer had been applied. Instead of a correction −1″·68 at 80° Z. D., we should have had −1″·75.—Ed.]

seeing how much sooner a bright star seems to transit than a faint star.

Mr. Gill. I thank Mr. Marth and all the other Members for their suggestions. I had no intention or wish to throw the slightest discredit upon the Greenwich observations. I had the good fortune to go through the Meteorological Observatory the other day, and saw the great care that was taken with regard to the comparison of the instruments; and with respect to the barometer which has recently been compared with Kew, the comparison was extremely satisfactory. A few ten-thousandths of an inch was all the difference that was found between them. Also when I applied to the Meteorological Committee, they sent me the best barometer and the best thermometer for the purpose of comparing the Cape instruments with the instruments in this country.

Mr. Talmage read a paper, including a letter from Professor Piazzi Smyth, commenting in very severe terms of censure on a paper by Mr. Sadler in the 'Monthly Notices' for January, entitled "Notes on the late Admiral Smyth's 'Cycle of Celestial Objects,' Volume the Second, commonly known as the 'Bedford Catalogue,'" in which reflections appeared to be made on Admiral Smyth's honour in publishing as his own observations several which the writer charged him with having copied from other astronomers.

The President observed that he had not stopped the reading of a paper which contained language considerably stronger than was usually heard at Meetings of the Society, because, on considering the matter, the Council had come to the conclusion that as an attack may, by implication, be made upon them for permitting the insertion of the paper in the 'Monthly Notices,' it was desirable to court inquiry as much as possible, and not to burke any investigation of the subject; and having said this, he would invite discussion, trusting that but little personality or strong language would be used, but that every thing might be kept as much as possible to Parliamentary language.

Mr. Knott was glad the subject had been brought before the Society. He had read Mr. Sadler's paper with a great deal of pain. Whilst Mr. Sadler had done good work by collecting together a number of cases of discrepancies and difficulties with regard to the stars in the Bedford Catalogue, many of which had been searched out before by other persons, it appeared to him that the real object of the paper was to throw discredit upon, and to make charges of falsification, forgery, and fraud against a thoroughly honest and honourable man. (Hear, hear!) He was sure that no one who knew Admiral Smyth would for a moment believe that he was capable of any such conduct. A scientific society had nothing to gain, but much to lose, if papers printed in its records were marked by offensive personalities and insinuations of unworthy motives. He trusted that by calling attention to the matter in this way there would be no occasion to make a similar complaint in future; and he very much deplored the insertion of the paper in question in the

'Monthly Notices,' the more so as the writer was a Member of the Council.

Mr. Gill assured the friends of Admiral Smyth that the feeling of the Council when the paper was printed was that it simply pointed out some of those errors which will arise in every thing that is done by human minds and human hands. The catalogue doubtless had serious errors. Errors might arise fraudulently or from want of care or want of experience. He would remind the Society of a notorious case of such imputation against a man who was one of the most honoured votaries of science that we have had. When General Sabine came back from his great magnetical series of observations he had made a set of observations with a small circle of some 6 inches diameter, which was provided with a level; and in determining his latitudes, Gen. Sabine devoted two or three pages of his work to the determination of the value of the level of this little instrument. Capt. Kater determined the value of the same level; and whilst Gen. Sabine's determinations gave something about 1″, Capt. Kater's careful determination gave something like 10″. Babbage made a most violent attack upon the honesty of Gen. Sabine, and declared that he must have forged his observations, because when he used the true value of the level which had been determined by Kater, the agreement of the latitudes *inter se* was very much worse than it was before. Dr. Robinson, who had told him the story, said that when Gen. Sabine was about to visit him, he did not know whether he should shake him by the hand as a true man who had done splendid work for science, or kick him out of the house. So he determined to get hold of the level and determine the value for himself; and he found it something like 20″. On examination he noticed that the level was cemented into a brass tube which held it, and which he thought might possibly have got into a state of strain in some way or other. He carefully removed the brass tube, and he found the value of the level to come out exactly what Gen. Sabine had determined it. He was sure that the Society would acquit Admiral Smyth of any unworthy motive. Struve had made errors and had confessed them, and Admiral Smyth, if he were living, would do the same; and Mr. Sadler, he was sure, would be the first to applaud and say that he was extremely sorry, and that in what he had said he did not mean to impute dishonesty, but only that some errors had crept into the catalogue.

Mr. Neison wished to draw attention to the fact that there was a great difference between the language of Mr. Sadler in his paper and the interpretation that had been put upon it by Mr. Talmage and others. There was no explicit charge of dishonesty; and the most that Mr. Sadler had said was that Admiral Smyth had copied his predecessors; but he (Mr. Neison) did not find it anywhere stated, or even implied, that Admiral Smyth forged those observations. He would read one paragraph, which was as follows:—"I have thought it better, therefore, as the charge I have brought against the 'Bedford Catalogue' is of a very serious character, to place an asterisk against the symbol of the observer whose erroneous measure Smyth appears to have followed." Mr. Sadler had only quoted Mr. Burnham's statement to show that there were numerous errors in the "Cycle;" and as the "Cycle" had been regarded as a work of such great accuracy that its measures had been used for the purpose of determining double-star orbits and other things, if there were, as had been stated, numerous errors in the work, it was only proper that those errors should be pointed out. He did not for one moment admit that Admiral Smyth would have done any thing which would be unbefitting the character of an English gentleman.

Mr. Lynn quoted Mr. Sadler's remark, "Smyth's observation is incomprehensible on the assumption that he really examined the star" (61 *Virginis*), and pointed out that such language was clearly inconsistent with Mr. Neison's interpretation of the paper.

Mr. Dunkin, as one of the older Members of the Society, and as a friend of the late Admiral Smyth, was glad to hear the acknowledgment on all sides that the Admiral was really a gentleman. (Cheers.) Mr. Neison had suggested that there was nothing to be complained of in Mr. Sadler's paper; but he must say that he holds a very different opinion. He had taken the trouble to go through a great many of the so-called inaccuracies given in that paper, and compared them directly with the 'Celestial Cycle,' and he found that they are mostly accidental errors which might have been easily made by any observer. (Hear, hear!) He must admit that during his forty years' experience he had made scores of mistakes of a similar kind; and he would be very sorry indeed if Mr. Neison, or any other Member of the Society, were to take the volumes of the Greenwich observations and say that all the corrections were dishonestly forged by him, as had been imputed to Admiral Smyth.

Mr. Sadler stated that he had not said so.

Mr. Dunkin would then take the very first case in Mr. Sadler's paper, in which he remarks that "Smyth says (Cycle, p. 12) 'The difference in distance is so remarkable that it must be imputed to instrumental error,' &c., and that the star was reexamined in 1851, when Smyth deduced a result agreeing with Captain Jacob's." Now an explanation of the cause of this error had already been given by Admiral Smyth, and it appears to have been owing to a mistake in the reading of the revolution of the micrometer. Why, therefore, has this known error been brought up to support the charge of dishonesty against the Admiral? In another case Mr. Sadler has said that an alteration of 100° had been made in the angular position of γ Persei. Why a mistake of 100° in the pointer-reading is the easiest thing in the world to make! What would have been more easy then to have transcribed a 2 for a 3? and as Admiral Smyth was observing faint objects with scarcely any light in the room, his error probably arose in this manner. He (Mr. Dunkin) had often made a mistake of 10°, 20°, 30°, or even 100° in the pointer-reading of the zenith-distance of a star; but Mr. Sadler being so young an observer, with so little experience, evidently had little notion of these mistakes to which all observers are liable. Taking another paragraph in Mr. Sadler's paper, he was very much grieved that it should have been allowed to be printed in the 'Monthly Notices.' It is this:—"As far as I am aware, there is one Catalogue only, and that not an original one, which surpasses the 'Bedford Catalogue' in inaccuracy, and that catalogue is the 'Reference Catalogue of Multiple and Double Stars,' forming vol. xl. of the 'Memoirs.'" He referred to this catalogue only as an illustration of the want of knowledge of Mr. Sadler as to what is and what is not an accurate catalogue. The whole of the errata to 6^h R.A., discovered by Mr. Burnham, had been submitted to him; and he had actually corrected the Observatory copy of Sir John Herschel's Catalogue, and really there are very few errors, and the pages are quite clean. If Mr. Sadler would come to Greenwich to-morrow he would show him several standard catalogues of stars; and he would then see that where there is one error in Sir John Herschel's Catalogue, there are a hundred in some others, especially in Weisse's first Catalogue of the zone-stars observed by Bessel; and therefore when Mr. Sadler said that the Herschel 'Reference Catalogue' is the most inaccurate catalogue that he ever knew, he (Mr. Dunkin) must come to the conclusion that Mr. Sadler's experience in such matters is worth very little.

Capt. Toynbee had had the pleasure and the honour of knowing Admiral Smyth, and he was quite sure that he was incapable of the dishonesty of putting down observations which he had not made. He had no knowledge of double stars, but he had great knowledge of men; and though the way in which Mr. Sadler—who was but at the commencement of his astronomical life—had put his remarks was very objectionable, he scarcely thought that he meant to convey the imputation which the words implied. If a midshipman of the present day were to abuse Nelson for not having steamed 15 knots an hour at Trafalgar, and for not having rammed the enemy, his conduct would resemble Mr. Sadler's attack upon Admiral Smyth.

Mr. Freeman thought, after reading Mr. Sadler's preface to his paper, that on the whole he had expressed himself temperately, and he had no doubt that in the future the 'Cycle' would not be used at all, or, if used, it would be as corrected by Mr. Sadler (Oh! oh!)

Mr. Christie suggested that instead of discussing the question from a purely personal point of view it would be better if some one would go through Mr. Sadler's criticisms of errata one by one, and review them dispassionately, and see whether they could be explained on any fair assumption as ordinary blunders of observation. To a large number of these observations Admiral Smyth gave the weight of 1, which according to Mr. Sadler, meant "nearly worthless;" but if Mr. Sadler brought those observations forward as casting a reflection on the general catalogue, that was a matter which might be discussed without introducing temper into the question, by pointing out fairly how much confidence was to be placed in these particular measures. There were 850 stars observed and 40 errata had been pointed out, to 20 of which Mr. Sadler had affixed an asterisk, as having been copied. It seemed inconceivable that a man like Admiral Smyth should have risked his scientific reputation by copying one-fortieth of the whole number of his observations. Having gone through a few of the cases, they appeared to be the sort of blunders that one was accustomed to find and to which all were liable; and he did not himself see any thing at all suspicious in the discrepancies. He only wished that some Fellow of the Society would take up the question fairly and discuss it thoroughly. (Hear, hear!)

Mr. Chambers, whilst approving of Mr. Christie's suggestion, thought it was due to the Society at large and to the Council that some understanding should be arrived at on the subject, or that Mr. Sadler should offer some explanation as to the real meaning of his paper. Unless he (Mr. Chambers) was very obtuse, as well as several gentlemen near him, they read the paper with the impression that there was a malignity about it and an intention to impute dishonesty and fraud to Admiral Smyth, which either ought to be explained or withdrawn. Whatever might be the result of the investigation suggested by Mr. Christie, it would not be satisfactory to him or to the Society at large if these imputations were not explained or withdrawn. (Hear, hear!)

Capt. Noble's experience of the 'Cycle' had been that its star-magnitudes had led him, in his juvenile days, to find more mares' nests in the heavens than any other work that he had ever opened. He believed them to be utterly untrustworthy. As an illustration of seeing things which had no existence he referred to the case of the assumed companions of Polaris which had been mentioned by M. de Boë, of Antwerp [laughter], and subsequently by a young Belgian nobleman, a boy who did not know what Polaris was, and also by Mr. Slack, the President of the Microscopical Society, and others; but Mr. Burnham with his 18-inch equatoreal declared positively that those companions had no existence whatever. As regarded the catalogue in Vol. xl. of the Memoirs, if Mr. Sadler instead of saying that it was a most inaccurate catalogue had said that it was the most useless one that was ever printed, he believed that all in the room would agree with him.

Mr. Talmage expressed himself as thoroughly satisfied with the discussion which had taken place, as his object and that of Professor Piazzi Smyth had been accomplished by the declaration of the opinion of the Society that Admiral Smyth was an honest man [Cheers]; and with the permission of the President he would withdraw his paper.

The President thought that the paper having been placed upon the table became the property of the Society and could not be withdrawn, but might be left without being printed.

Mr. Chambers thought that Prof. Smyth's paper was couched in fair and reasonable terms, and he begged to move that it be printed.

Mr. Sadler begged to second that proposition.

Mr. Ranyard thought that the Society were agreed that Admiral Smyth had performed very great services to astronomy [Hear, hear!], that his papers had interested a very great number of men who are now old, and that the Society did not wish to take away his character; but there could be no doubt that it was of very much advantage to the Society that there should be a free criticism of the whole question at issue, and if there were no warmth on either side there would be less chance of any malignity or malice being suggested.

The President. The question of criticism is one of very great and vital importance to our Society. I cannot help thinking myself that had Mr. Sadler's paper been devoted to a mere numerical list of errata, and the corrections which were to be made in the 'Cycle,' it would have been of great importance, and the value of the book would have been enhanced; and perhaps to Mr. Fletcher, from whom we have been hoping for many years past to have a new edition, it would be a very valuable thing. Such lists of errata were prepared and issued by Argelander when he was compiling his catalogue of stars. He merely issued lists of errata in the catalogue that he was examining without imputing any motives and without putting any remarks, which no doubt is the proper method of correcting tables. I think if Mr. Sadler would give his attention to completing the tables of errata of the various catalogues of astronomy that we have, it would be an extremely valuable service. No one would value such a table more than I should myself. I would suggest that he might take up the B.A. catalogue.

I think the discussion which has been brought forward by Mr. Talmage's paper is one which is of value to the Society. It is decidedly a good thing to have this ventilation of opinion respecting an old and valued member of the Society, such as Admiral Smyth doubtless was. I never had the pleasure of his acquaintance and often regretted it; but I must say that the 'Cycle of Celestial Objects' was one of the first treats that I ever got in astronomy, and the delight which I felt in reading it did more to turn my attention to the science than any thing else; and I believe I am no solitary instance of that, for I know no book in any language which has had more influence in directing the attention of amateurs toward the science.

Capt. Toynbee considered the Chairman's suggestion a good one, and thought that it should be inserted in the minutes.

Mr. Sadler simply wished to state that he had not imputed dishonesty to Admiral Smyth *, but he certainly had a right to point out errata. It was quite possible that Admiral Smyth had made mistakes; but if Herschel gave a position-angle of 224°·0 and Smyth gave 224°·1 *, when the actual angle ought to be 324°, and if that mistake had been copied scores of times, any one would conclude that Admiral Smyth's observations were influenced by some bias. M. Flammarion assigned a proper motion of something like 4″ per annum to θ Persei on the authority of Admiral Smyth's observation, which was perfectly erroneous, and it rested on the observations of Herschel. Instead of about 60″ Smyth gave 27″, which was an entirely improper measure assigned to θ Persei †.

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* [It is not easy to reconcile this statement with the following passage from a letter in the 'English Mechanic,' signed by Mr. Herbert Sadler, and published on the very day of the R.A.S. Meeting:—"When the Admiral died the 'Cycle' had been published more than twenty years, ample time for the detection of the

Dr. Copeland observed that Mr. Sadler had stated that Admiral Smyth had copied or followed Herschel in scores of cases, but he had not shown those scores of cases. Errors might arise on all sides although the observer might have the best possible intention. A position-angle might be given as 173°; Sir John Herschel might have observed 137°, and Admiral Smyth from his own observation might have got 173°, but on transcribing he might accidentally transpose the figures. He offered that remark as showing how a man may appear to have copied the observation of another although in perfectly good faith. It was perfectly absurd to imagine that any man who could produce a work like the 'Cycle' could forge one-twentieth or one-fortieth of the observations. A man might assume an observation to be in error a minute of time, or ten minutes, or two minutes in declination and two in right ascension; or he might make scores of hypotheses if necessary, but he should never think of couching his remarks in such language as to throw the slightest discredit upon the observer. He thought it but right, as those remarks with respect to Admiral Smyth had been printed and circulated to all the world, that something should be printed in the 'Monthly Notices' to show that the Society did not agree with the statements in Mr. Sadler's paper.

Mr. Talmage stated that the serious charge was that the printed publication did not agree with the manuscript notes of observation.

Mr. Common thought that as no doubt there were many errors in the 'Cycle,' if Mr. Sadler would withdraw any imputation of bad

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errors; and it does not argue well for the state of astronomical scholarship that such a 'stupendous fraud,' as one of the first of double-star observers has happily termed it, should have remained so long undiscovered."—Ed.]

* [The difference between Herschel's and Smyth's position-angles is 1°·1, not 0°·1 as Mr. Sadler states, the actual figures for position-angle being 224°·9 and 226°·0, while those for distance are 60″ and 55″·0. It is unfortunate that Mr. Sadler should have altered the figures in the position-angle and ignored the discordance in distance; for the presumption that the observations had been copied rests on the agreement within a fraction of a degree and of a second in both elements.—Ed.]

† [*Cf.* 'Observatory,' No. 8, p. 245, where M. Flammarion points out that Smyth may have written 27″ for 67″. There is, however, no evidence whatever that Smyth's observation is erroneous. M. Flammarion found an annual proper motion of not quite 1″ instead of 4″ as Mr. Sadler states.—Ed.]

faith on the part of Admiral Smyth all ground for personal feeling would be removed, because as legitimate criticism no one could object to the paper.

Mr. Sadler replied that he could not retract the grounds of his imputation, as he had given them in his paper and it must stand or fall by them.

Mr. Ranyard supposed that what Mr. Sadler had said might be interpreted by the Fellows in any manner that they chose, but he had not said that he imputed any ill faith.

Mr. Sadler maintained that he had not said so. He had pointed out the remarkable coincidence between the observations, but he did not know that any one could say that he had imputed dishonesty to Admiral Smyth. He would be far from saying that Smyth had copied those things.

Mr. Christie called attention to the fact that Mr. Sadler had distinctly said that they had been copied, and that he had noted with an asterisk the observations from which Admiral Smyth appeared to have copied them, and all through the list of errata these observations have been so marked. He did not see how Mr. Common's suggestion could be practically carried out, because the whole paper would stand or fall upon that point.

Mr. Neison submitted that Mr. Sadler did not say "copied," but "appears to have followed;" and he (Mr. Neison) should not have given the interpretation to certain words in the paper which had been placed upon them by some of the Members present.

Mr. Christie thought if Mr. Neison had read a little further he would have seen that Mr. Sadler had said "so that any one may be able to detect the source of Smyth's error at a glance in cases where he has presumably *copied* the measures of others."

Mr. Gill suggested to Mr. Sadler that he should withdraw the word "copied."

The President inquired whether it was the pleasure of the Meeting that Mr. Talmage's paper be withdrawn. (Cries of No, no!)

Mr. Penrose said that it appeared to him that it was impossible to acquiesce in the desire to withdraw Mr. Sadler's paper, unless it were explicitly stated by him that the insinuation which was the natural interpretation of his paper should also be withdrawn. If he did not do so something ought to appear in the Proceedings of the Society to denote that they were not themselves of opinion that Admiral Smyth palmed off the observations of others for his own.

Mr. Ranyard observed that the Society does not endorse by any means all the statements in the papers that it prints, and it was most important that there should be the utmost liberty of criticism. Of course it was open to critics, on the other side, to defend their friends; and in the present case the Society was not pledged to any assertion made in Mr. Sadler's paper. He concluded that when it was allowed to be printed it was not considered by the Council to be sufficiently unparliamentary in its language to prevent its publication.

Mr. Talmage pointed out that at p. 191 of the 'Monthly Notices' Mr. Sadler had said,—"Smyth mistook Herschel's *Serp* (i. e. *Serpentarii*) for *Serpentis*, and has *measured*, therefore, a companion which has no existence, and, moreover, was careful that his measures should agree with the supposed prior observation of Herschel. It is noteworthy that these *measures* are stated to have been made in the year 1835, in which year he did not make any measure of 39 *Ophiuchi*. Smyth actually gives the colour of the star he did not observe!" *

The President put it to the meeting that Mr. Talmage's paper be not printed in the 'Monthly Notices'—which was negatived by a large majority.

The President then introduced Professor Langley.

Prof. Langley. I was requested by the Superintendent of the Coast Survey of the United States to make certain observations whilst I was in the Mediterranean on the character of astronomical vision at different altitudes. Unfortunately I did not reach Etna until the snows had set in. Climbing on Christmas day into a region some three hours' journey beyond the furthest inhabited house, I took up my quarters under a shed not unknown to some members of this Society, for it was occupied by one or two of them in June 1871. My observations there led me to believe that the advantage of mountains, as far as regards steadiness of definition, had been much exaggerated; but as regards light, there was an unquestionable gain. My own eye is not at all sensitive to faint objects, and it would be very unfair to compare what I saw there with what such an admirable observer as Mr. Dawes could see here; but I found that I could see with about the same apertures there what very much better observers than myself of such objects can see here. In the clear atmosphere of Sicily my brother, who has a better pair of eyes for such objects, discerned steadily nine of the Pleiades with the naked eye, and glimpsed two or three more. At this station, halfway up Etna, I saw very distinctly nine stars, and glimpsed almost any number beyond. The general conclusion was, that as regards faint objects by night, like the nebulæ, the gain was very marked; and also in the daytime as regards questions connected with solar physics, but most especially as regards the chromosphere, the gain was enormous. It is hardly too much to say, in spite of all we know of unfavourable circumstances, that the future of solar physics is very largely bound up in this country with observations in a more elevated atmosphere. But having said that, I would like very much to suggest that some expression be arrived at (and this is especially a matter on which we want opinions from all sides) as to what does constitute conditions good for seeing. After great experience in the

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* [Smyth made three observations of ♅ III. 25; one of these he entered, by an obvious slip, as 39 *Serpentis*; the other two, correctly, as 39 *Ophiuchi*. If Mr. Sadler had had the slightest acquaintance with Observatory work, he would have known that mistakes in the names of stars, leading to double entries, are everyday occurrences.—Ed.]

Rocky Mountains in the west of America, some in the Southern States, some in Egypt, some in Italy, and some on Etna, I think I am nearly justified in the position that generally we want a very dry climate. I prefer a dry climate in a very elevated region; but the first gentleman that I spoke to, whose opinion was authoritative on such subjects, especially on solar work, declared that he was in favour of a climate in which the air was quite saturated with moisture, provided it did not present itself in the form of actual cloud. I have elicited some very different opinions in the course of my inquiry while in Sicily and in London, and the opinion was expressed that the climate at Greenwich was better than that of Columbia—a very singular statement, because we are all under the impression that the great work which has been done there has not been done on account of the purity of the air. And generally I find almost every gentleman inclined to think that almost any other station or observatory than his own is more favourably situated. I have met with such a variety of opinions on the subject that what little conclusions I thought I had arrived at seem to be entirely unsettled; and it is for that reason that I have ventured to trespass upon your time tonight, in the hope of eliciting some contributions towards forming a better judgment in my own mind from that of gentlemen with wider experience.

Mr. Brett. Professor Langley says very modestly that his eye is not particularly sensitive to delicate objects, and yet he can see the planet Mercury on the corona several minutes, I think, from the limb, and can see the corona for 15 minutes in broad daylight—an observation which was stated to this Society some years ago, and laughed at from one end of the room to the other. I am delighted that Prof. Langley is here this evening to corroborate what I have said before, viz. that you can see the Sun's corona in broad daylight.

The President. I do not know whether Prof. Langley can say that he can always see the corona of the Sun in broad daylight.

Prof. Langley. Not always in broad daylight. I have thought I have seen evidence of it in daylight. I believe I saw Mercury just before it reached the limb, which seems to me to be evidence of the same kind.

Mr. Christie. Prof. Tacchini, at Palermo, describes his seeing the corona in full sunlight, not only in the immediate neighbourhood of the Sun, but to a great distance from it, and on the very day of the late Eclipse of the Sun. He gives a drawing in the memoirs of the Italian Spectroscopic Society agreeing fairly well with the drawings made during the eclipse, and he attributes his success to the exceeding purity of the air at Palermo on that particular day when the dry sirocco wind was blowing.

Mr. Ranyard. No doubt a good many observers have thought they have seen the corona in some form chiefly near the Sun; but some of the drawings of the corona made by such observers are certainly very unlike some of the drawings made during eclipses. I think the crucial test is whether such objects as the Moon can be seen on the background of the corona. We hardly know what scattered light there may be throughout the atmosphere. It seems certain that the Moon is not seen before it comes up to the Sun's disk in ordinary eclipses; but after it has come up to the Sun's disk and has cut off a very large portion of the light of the photosphere, there is very conflicting evidence as to whether even for a minute or so the Moon's limb can be seen outside the Sun. My own conclusion, from a great number of observations is, that occasionally it has been seen, but certainly not for 10 or 15 minutes, and possibly it may have been when the Moon has been passing over prominences on the very much brighter parts; but I should say that the evidence was entirely against the Moon's limb having been seen upon the corona.

Mr. Neison. Might I ask Prof. Langley whether it is not possible that when Mercury was seen projected upon the corona, it was simply the atmosphere reflecting the light? for, as we know, in the case of Venus, it was seen 36 hours before the transit, which must have put it a very considerable distance indeed from the Sun, and quite out of the range of the corona.

Prof. Langley. The circumstances under which I saw Mercury were altogether exceptional. At the moment of the first contact there was the bluest sky that I had seen in the year at Allegheny; and the reason why I believe that I was not biassed was that I was using a polarizing eyepiece, which changed the position-angle as I turned it, so that I entirely lost my position, and I did not know within 15° on either side where the planet was coming on. Under these circumstances I distinguished Mercury in contact with the limb just discernible on a background of barely brighter sky. I revolved the eyepiece in order to make certain that no speck of dust or illusion arose from the eyepiece; and I can hardly doubt that there was really an observation there which indicated something sufficiently bright to give that background.

The President. I think the Society will be extremely grateful to Prof. Langley for giving us an instance so thoroughly well recorded of a very rare phenomenon.

Mr. Ranyard. In a very faint light there is great difficulty in saying which of the two areas is the brighter; and I think that an observer ought to speak with very great caution, when he says that an area is marked out either by an outline, as it would be in the case of light reflected in the atmosphere of Mercury, or that it is marked out by an outer background rather brighter than the inner disk of the planet.

Capt. Noble. I have stated more than once in this room, that on two occasions when observing eclipses I have seen a projection of the Moon's limb to the extent of 1′ upon a bright background. Upon the first occasion I had a dark blue glass, and then the President of the Society told me that that would account for any thing. I tried upon the next occasion a glass of an absolutely different colour, so that no chromatic phenomena interfered, and there was a visible projection of the Moon's limb upon something brighter —I do not profess to say what—in the neighbourhood of the planet.

The following papers were announced:—

J. Tebbutt. Observations on Encke's Comet, 1878.

Prof. A. Sawitsch. Les longueurs du pendule à seconde à Poulkova, à St. Pétersbourg, et aux différents points de la Russie occidentale, corrigées de l'influence produite par la flexion des supports du pendule construit par M. Repsold.

Prof. A. Krueger. A formula for reducing precession in Right Ascension and Declination from Bessel's to Struve's Constants.

E. L. Trouvelot. Observations of absorbing vapours upon the Sun.

W. T. Lynn. On the change in the errors of Hansen's Lunar Tables between 1848 and 1876.

W. T. Lynn. Note on η Draconis.

A. Schuster. On the probable presence of Oxygen in the Solar Chromosphere.

Prof. C. Pritchard. On the computation of the heights of Lunar Mountains from Photographic measurements.

J. Birmingham. Note on Gruithuisen's Lunar crater "Schröter."

The following were balloted for and duly elected Fellows:—

Maures Horner; E. E. Markwick; A. E. Nevins; C. C. Walker.

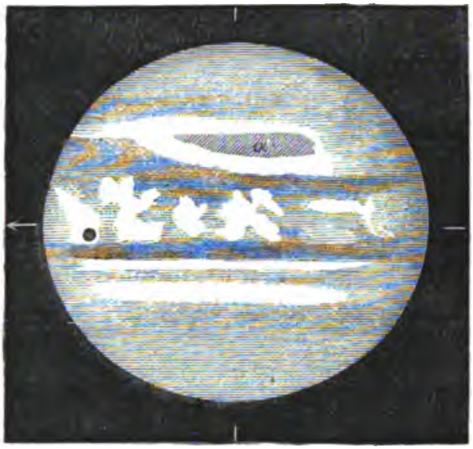
The meeting adjourned at half-past ten o'clock.

On the Recurrence of some of the Markings on Jupiter.

Professor C. W. Pritchett having lately published in the 'Observatory' the account of a remarkable red spot seen by him on Jupiter, on the night of July 9, 1878, it may be of some value to record the observations of a similar phenomenon seen later in Cambridge, U.S., especially as the two phenomena seem to be directly connected.

While observing Jupiter on September 25, 1878, at 6^h 30^m C. M. T., a very remarkable red spot was seen just a little above the southern edge of the equatorial belt, its centre being then situated a little to the east of the central meridian. This curious object, which apparently occupied one fifth of the planet's diameter, was very conspicuous, its intense rose-colour appearing in strong contrast with the white luminous background on which it was projected. It was of the same uniform shade throughout, without any dark border, its vivid rose-colour forming the whole spot. It appeared isolated from and perfectly independent of the equatorial belt, from which it was separated by a brilliant white band. In shade, the colour of this spot differed totally from the pale pinkish colour of the equatorial belt, or from any thing I have ever seen on Jupiter; a mixture of vermilion and white would very nearly give the shade of this mark.

The annexed figure is a copy of the original drawing made immediately after the observation, α being the red spot.



Since this observation was made, the return of the same spot has been observed and drawn fifteen times, it having been last seen on December 30; after this time it became impossible to follow it any longer, on account of the proximity of the planet to the sun. The form of this spot changed somewhat during this period, as when first observed it was narrow and elongated, while at last it was shorter, much wider, and extended further in the south.

It is perhaps a very curious fact that this object corresponded very nearly in position with the rose-tinted elliptical cloud observed by Prof. Pritchett on July 9, or 77 days before I saw it for the first time, the planet having rotated very neary 188 times during this interval. However, it was certainly not the same identical spot seen by Prof. Pritchett, since he was unable to see it again on July 10 and 15, when it should have been visible if it still existed; while I also failed to see this spot on September 6, 10, 15, 20, and 22, when Jupiter presented the same region to the observer. Between September 19 and 20, considerable changes occurred in the markings of the planet, the southern border of the equatorial belt assuming at one point a very characteristic angular form, which on the 25th was recognized on the west of the red spot and quite close to it. If the red spot had existed on the 20th, it is impossible that it should have escaped unnoticed, as the atmospheric definition was very fine on that night, and a good drawing of Jupiter was obtained. It seems certain therefore that this spot formed between the 22nd and 25th of September, as it was not seen on the 22nd at $7^h\ 0^m$ C. M. T.

It is very remarkable that this spot appeared precisely at the same place, or at least very nearly so, where a similar object had been observed over two months before. The recurrence of some of the markings of Jupiter at the same places where similar forms had already been observed previously, although quite a rare phenomenon, is not, however, without precedent, as appears from my observations of this planet. For instance, the angular marking on the southern border of the equatorial belt described above, and near which the red spot made its appearance, very strikingly illustrates the phenomena of recurrences, since this spot so familiar to me has disappeared and reappeared three times in a period of observations embracing a little over one year.

Perhaps it is worthy of remark that the two markings on which I have observed a recurrence were situated very near each other.

The angular marking of the southern edge of the equatorial belt was not seen once in 1876, during the 130 nights on which Jupiter was observed; it was first seen on April 14, 1877, on the second day of my observations of this planet in that year. It continued to be visible for six months, until September 14; after which date the markings of this part of Jupiter changed and appeared totally different. On September 16, 18, 23, the planet presented the same side to the Earth; the definition was good on these nights and the sky clear, but the characteristic spot was not seen. However, on September 26, twelve days later, the same remarkable form was found in the same place, and it continued to be visible for forty-one days longer, until November 6. The same part of the planet was observed on November 13, 15, and 17; the angular spot was not there, but a straight uniform border in its place. Twelve days later, on November 19, a similar form had reappeared in its place, with its characteristic features. The observations of Jupiter were abandoned on December 10, when the planet was too near the Sun, and they were only resumed systematically on September 6, 1878. On the 8th of that month, an angular marking, resembling exactly that observed the previous year, was seen on the same part of the southern border of the equatorial belt. On September 10 and 15 it should have been seen, as the planet presented the same side to the observer, but in its place the border of the equatorial belt was straight and uniform. Strange enough, twelve days later again, on September 20, a similar form reappeared again in the same place, as stated above; and from this time it remained unaltered and visible until the close of my observations on January 12, 1879, when Jupiter was too near the Sun to be observed. The return of the angular marking has been observed and drawn 62 times during the twelve months that Jupiter was observed on every clear night.

This singular phenomenon of recurrences would seem to indicate that local causes participate in a certain measure in the production of the markings on Jupiter, while the recurrence of the same form on three different occasions, twelve days after its disappearance, would seem to indicate a periodic action in these causes. If it were proved that local causes sometimes give the markings on Jupiter, such spots would best give the period of rotation of the planet, as they would be less apt to have a proper motion.

Cambridge, U.S.A.,
1879, Jan. 20.

E. LÉOPOLD TROUVELOT.

Schmidt's Lunar Map.

Closely following the appearance of Lohrmann's complete sections, edited by Dr. Schmidt, we have this author's own lunar map published by the Prussian Government. Dr. Schmidt's great work, which he calls a chart of the lunar mountains, is, like Lohrmann's, in 25 sections, which would form a map of six French feet diameter, about 6 feet 6 inches English. The sections, however, are intended to remain separate, each one forming a map in itself, graduated at the edges, and they are thus evidently more convenient for reference than if joined together. They are in photo-lithograph, made from the original chart at the office of the Prussian General Staff.

It redounds highly to the honour of his Imperial Highness the Crown Prince of Prussia that it was at his request, and under his auspices, that the publication of the work was undertaken by the Government, which has thus been the means of giving to the world a performance that must be placed among the greatest feats of astronomical science and observation, while it must rank among the most distinguished achievements of German capability and perseverance.

In the volume accompanying the map, Dr. Schmidt gives a history of his labours, which may here be briefly referred to.

In the autumn of 1839, when of the age of 14 years, and at his own home at Eutin in Oldenburg, a copy of Schröter's work on the Moon came into his hands, and he was so struck with the figures of the shadow-throwing hills and craters, that the effect in a great degree fixed the bias of his after life. His own first observations were made with a small telescope, the work of his father; and with the instrument steadied by a lamp-post, he made his first attempts at sketching. The study of Schröter's work, together with observing and sketching, now became his chief employment, and it was not long before he constructed a rough wooden stand for his telescope, which greatly facilitated his observations. At length, owing to the kindness of a gentleman, who noticed the industry of the young astronomer, he procured, early in 1841, the loan of a 4-foot Dollond, with a power of 15 to 20. In July of the same year he saw, for the first time, the Moon in a larger telescope. This was at the Altona Observatory, where Petersen showed him the craters Bullialdus and Gassendi; and now he got a fair idea of the rich abundance of lunar objects, both by his observations with the large instrument, and still more by what he saw in Mädler's chart, which he found at the observatory. In 1842 he came to Hamburg, and through the goodness of the director of the observatory, he had the use of several telescopes from 1842 to 1845. In 1845 he came to the Bilk Observatory, where he made but little progress in his lunar sketches.

It seems worthy of remark that the scientific ardour of the worthy director of this observatory, Herr Benzenberg, was just sufficient to allow the young astronomer the use of only a wretched instrument, while the principal telescope was rigidly tabooed, so that its outward good looks and polish might not suffer by handling! Very different was the conduct of those able men, Galle and Bruhns, who subsequently placed the great Berlin refractor at Herr Schmidt's disposal on several occasions. From 1845 to 1853 he was employed at the Bonn Observatory, where, on account of other work, he was not able to pursue his lunar studies in the usual manner, though still he succeeded in making many useful sketches. From 1853 to 1858, at the Olmütz Observatory, he made not many drawings, but numerous micrometrical measurements. In the interim, in March 1855, he sketched at the great Roman refractor, and in April at the observatory at Naples.

On December 2, 1858, Herr Schmidt came into his present position as director of the Athens Observatory, where the condition of things prevented all observing for a whole year, until the 6-foot refractor by Plössl was again set up. From February 1860 to the beginning of 1865 considerable progress was made in lunar observations, although the greater part of the time was occupied with other work.

At length Dr. Schmidt determined to lay down all his fragmentary surveys on a 6-foot map, for the purpose of seeing what parts of the Moon had been neglected in his work, which was principally expended on certain favourite areas. All this time, however, he had no notion of completing any general chart, not seeing that he could ever bring to an end the delineation of the lunar surface as it appeared in the 6-foot Plössl. Simultaneously with this, in 1865, he seems to have begun a map of 1 metre diameter, in 4 sheets, in which the principal objects were laid down from Mädler, and the details of his own observations added in. He now very soon found how much he wanted and how little he possessed; and he was obliged so to extend and repeat his observations, that the older ones became at length of little importance.

Nine years longer were devoted to this work, until, in July 1874, he determined on concluding it, as it became manifest that for an exhaustive representation of all the details of lunar features, visible in a 6-foot refractor, would be required more powers of endurance and a longer lifetime than are allotted to mortals.

Dr. Schmidt seems not very clear on the subject of these maps. For two years, he says, he worked at the first great map already referred to, and added from time to time all the results of the Athens Observations, until it became evident that this would lead to no satisfactory conclusion. The division into four parts was inconvenient for several reasons; and Dr. Schmidt finally determined on a 6-foot map as before, but divided into 25 sections, corresponding with Lohrmann's, but of course, on double the scale. The graduation was copied from Lohrmann; but it was afterwards discovered that it would have been better if this work had been done specially for the new map. If Lohrmann's sections contained faults of graduation, on account of the paper suffering unequal pressure in the printing, the errors in Schmidt's would be somewhat greater, as not alone adopting the faults of the original, but suffering from imperfections of their own, induced by the unequal expansions of the different papers on which the sections were drawn. But this, after all, was of little import. Although the margins of the sections might not close so well together as was desired, still the delineation of details succeeded in the main as intended, and greater faults were happily avoided.

The construction of this second map was continued without intermission, except from August 1869 to April 1870, and in July and August in each year, and meanwhile every favourable time for observation was made use of. In 1873 and 1874 the non-topographical part of the map (that is, the colouring) was added.

It was found that in the course of eight or nine years' working at the sections, the parallel and meridian lines were obliterated, and an attempt in 1874 to restore them was unsuccessful, because in the mean time the surface of the paper had become too rough. Each section has, however, the graduation at the margins, so that approximate positions can be determined. For several considerations, it was decided to write no names in the sections, but to confine them, in number over 500, to the descriptive letterpress.

In 1874 the author brought his chart to the observatory at Berlin, where it excited so much interest that it was determined to publish it under state patronage. At the instance of His Imperial Highness the Crown Prince, the 25 sections were photographed at the General Staff Office, under the direction of Count von Moltke; and Dr. Schmidt, having received proof-sheets in April 1875, was thus assisted in proceeding with the descriptive text, which he had commenced in 1873. Only the observations from 1842 to 1874 have been made available for the chart. The sketches of previous years were considered imperfect, and were rejected.

J. Birmingham.

[To be continued.]

Double Stars for April.

From R.A. $7^h\ 33^m$ to 9^h the following pairs are the most important.

Pollux. R.A. $7^h\ 37^m{\cdot}9$, Dec. $+28°\ 19'$.

Mag. A 2, B 14, C 11, D 12, E 10.

		°	″	
A B	1877·95	274·9	43	Burnham's companion.
A C	·08	72·1	175·0	Flammarion.
A D	·08	90·4	205·5	,,
A E	·08	75·2	228·9	,,

Flammarion observes that C is probably a variable, inasmuch as it was not seen by H and So. DA. and Sm. saw it in 1832.

The principal star is remarkable for its large proper motion. Mr. Burnham has lately found that B is double; his measure is 128°, 1″·4, 1878·19.

Σ 1196 = ζ Cancri. R.A. 8^h 5^m·3, Dec. +18° 1′.

Mag. A 5, B 5·7, C 5·5. Colours yellow.

		°	″	
A B	1877·18	108·2	0·82	Spörer.
	·17	·7	·68	Dembowski.
$\frac{AB}{2}$ and C	1877·32	131·2	...	Doberck.
	·29	·7	5·29	Dembowski.

The star C is remarkable for the irregularities of its curve, several loops having been described since 1827. OΣ was the first to notice and investigate these curious epicyclic curves (see Pulkowa Obs. vol. ix.). M. Flammarion noticed and examined them in 1873.

The period of B appears to be about 60 years.

Σ 1273 = ε Hydræ. R.A. 8^h 40^m·4, Dec. +6° 52′.

Mag. A 3·8, B 7·8. Colours, A yellow, B blue.

1876·23 218°·5 3″·5 Gledhill.

A beautiful pair. Binary. Common proper motion −0^s·013 in R.A., and +0″·04 in N.P.D.

The position of Hall's new companion is about 190°, distance about 12″. Burnham gives the following:—

1878·00 192°·2 14″·74.

OΣ 1306 = σ^2 Ursæ Majoris. R.A. 8^h 59^m·8, Dec. +67° 37′.

Mag. A 5, B 8·2.

1876·2 244°·8 2″·7 Gledhill.

Much uncertainty attaches to the conclusions drawn from the measures of this object. It is probable that the two stars form a physical system. JOSEPH GLEDHILL.

Meteor Notes for April.

THE last half of the month is available for A.M. watches, and from the 8th to the 26th for P.M. watches. The *Lyrids* of the 19th–20th are the most important shower, though since the maximum in 1863–4 it does not appear to have been well observed. The exact agreement of its radiant-point with that of its allied comet (I. 1861) is a little questionable; hence we especially require further observation of these meteors, and an accurate redetermination of the radiant. There are a large number of other showers visible at this epoch, several of which, situated eastwards of the *Lyrids*, have only recently been found. A projection of the paths of about 300 shooting-stars (apparently directed from such radiants) recorded in foreign catalogues (chiefly that of Dr. E. Weiss, which includes a large proportion of April meteors) indicated the following centres for April 19–23:—280° +58° (22 meteors), 295° +42° (25), 288° +22° (20), 309° +36° (30), 316° +61° (16), 309° +68° (15), 284° +44° (11), 312° +22° (13), and several others of minor importance. The first of these coincides with Mr. Greg's *Draconids* II. at 281° +57°, April 20–May 25. The next two are new positions; of the remainder, the centre near ε Cygni is the most active and corresponds with Mr. Greg's *Cygnids* I. at 305° +37°, March 15–April 20. Westwards of Lyra many other radiants are given in the catalogues of meteor observers, including positions at 204° +56°, 235° +23°, and 258° +37°. The latter was well seen by Zezioli during the first half of the month, and is a shower distinct from the *Lyrids*. Fireballs should be expected on the nights of the 11th–12th, which shows a very decided epoch of intensity of these phenomena. The major radiant (found by a projection of many bright meteors seen on these dates) is in the Lynx at about 106° +46°, with a bordering shower at 121° +41°, and others in the fore part of Ursa. Von Niessl determined a radiant of two fireballs on April 9–10 at 18° +57°. The occurrence of a special shower of some intensity is to be inferred from the large number of fireballs recorded on about the 11th–12th.

Recent Observations.—Mr. J. L. M^{c}Cance, of Putney Hill, London, observed a meteor = Rigel at 6.43 P.M. on Feb. 20. Path from 25° +40° to 32° +35°; colour white, with a yellowish tinge. Near the end of its track a smaller luminous body was projected some 5′ or 6′ in front of the nucleus. Duration 2–3 secs.

The York 'Herald' records the appearance (on Feb. 24, shortly before 3 A.M) of a pear-shaped ball of fire in the northern heavens, which travelled slowly downwards towards the horizon and emitted scintillations and a light of great brilliancy almost equal to that of day. Afterwards a sound, described by some as resembling the discharge of heavy cannon, and by others as that of the rumbling of distant thunder, was heard; but in all cases it seems to have been sufficiently violent to rattle windows &c., and to have raised various speculations as to what could be the cause, some ascribing it to an earthquake, others to lightning, while others who saw the meteor set it to the account of an unearthly visitor.

On March 2, at 6.15–20 P.M., a fireball was seen at Winchester, and described to Mr. M^{c}Cance as having an apparent path from 70° −20° to 85° −25°, but the end point was hidden by trees. The meteor appeared quite round and about 1-3rd the size of the full moon. It was yellowish in the centre and red towards the circumference.

March 8, 7.20–25 P.M., fine meteor = ♃, seen at Bristol. Alt. 18° in S.S.E., descending obliquely towards S.E. Rather long path. Train; rapid.

March 9, 8.30 P.M., "a meteor of considerable magnitude appeared in the northern sky (at Bristol), passing from east to west;" and on March 12th another brilliant meteor was seen there at 7.35 P.M., with a path approximately observed at 210° +40° to 197° +7°; slow motion; train. The same meteor was seen by Mr. Christie at Blackheath near London at 7.37 P.M., and the recorded path is almost identical with that noted at Bristol, viz. from 205° +35° to 196° +6°. At Blackheath it was rated = or > ♀. Fine meteors were also seen at Bristol on March 7, 13, and 17.

W. F. DENNING.

CORRESPONDENCE.

To the Editor of 'The Observatory.'

The Spectrum of γ Argûs.

SIR,—I measured the bright lines in the spectrum of γ Argûs on two evenings very carefully, and thought perhaps it might be of some interest for 'The Observatory.' Only three lines could be measured, and only three bright ones could be distinctly seen; others suspected were evidently bright spaces only.

Yellow	1,	wave-length	5760.
	2,	" "	5648.
Blue	3,	" "	4682.

Wave-lengths according to Ångström's Normal Spectrum; Grubb spectroscope on Melbourne reflector.

I am, Sir, yours faithfully,

ROBERT L. J. ELLERY.

Melbourne Observatory,
1879, Jan. 24.

Brorsen's Comet.

SIR,—I have the honour to communicate to you, at the request of M. Tempel, the following observations of Brorsen's comet:—

	Florence Mean Time. h m s	R. A. h m s	Dec. ° ′ ″	Stars.
March 10....	7 27 59	1 28 29·82	0 31 38·5 S.	*a*
10....	7 43 4	1 28 32·74	0 31 7·7 S.	*b*
14....	7 11 21	1 41 46·54	2 54 19·2 N.	*c*
14....	7 35 56	1 41 49·20	2 55 37·9 N.	*d*

Comparison stars:—*a*, Börg. Cop. 353–4; *b*, W. B. i. 491; *c*, Schj. 530; *d*, Lal. 3230–1.

The second and fourth observations deserve, according to M. Tempel, more weight, the comparison stars being better situated with respect to the comet.

I am, Sir, yours faithfully,

L. SCHULHOF.

Paris, 1879, March 23.

NOTES.

Changes in the Vertical[*].—In making some observations with a sensitive level on the shore of the Lake of Geneva, M. Plantamour noticed remarkable changes in the course of the day, the east end rising steadily from about 10 A.M. to 4 or 5 P.M. on three successive days, the extent of these oscillations being $8''\cdot4$, $11''\cdot2$, and $15''\cdot75$ respectively. As the foundations of the building had only been laid a year and were perhaps too near the lake, M. Plantamour repeated the observations in his cellar, which has a solid foundation of concrete, and in which the temperature hardly varies, ranging between 13° and 13°·5 Centigrade during the whole of the observations. Similar results were obtained, there being at certain periods a gradual elevation of the east without sensible return, at others a certain degree of fixity in the horizontal plane, and finally, for periods of greater or less duration, oscillations of the soil, sometimes in the east and west direction, sometimes in the north and south, the greatest of which did not amount to $20''$. It is important to mention that observations by M. Turretini at a station 3 kilometres distant, show that the continuous elevation of the east went on simultaneously at the two places.

M. d'Abbadie, with reference to this communication of M. Plantamour, gives a summary of his own and other observations of changes in the vertical. The most important of these were described in 'The Observatory,' N° 14, and an account of his 'Nadirane' given. It is interesting to note that though M. d'Abbadie's observations were made within 400 metres of the sea-shore, the changes in the vertical cannot be explained by the action of the tide, for out of 359 consecutive high and low tides observed, only 243 are favourable to the attraction of the flood, whilst of the 116 others there are 57 for which the waters of the ebb would appear to attract the plumb-lime. M. Bouquet de la Grye at Campbell Island found that the heavy sea from the south made itself felt by a deviation of $1''\cdot1$ in the vertical. On one occasion the plumb-line changed $3''\cdot2$ in three hours and a quarter. With reference to the enormous fluctuation of $17''$ found by M. Plantamour, M. d'Abbadie asks whether this may be a result of the proximity of the Alps, the more recent formation of which might lead us to suppose the immobility much less absolute.

Variable Stars[†].—Dr. Schmidt, of Athens, gives an account of his observations of a number of variables in 1878, including a new one in the Orion-nebula. This star, which is $34^{s}\cdot3$ *f* and $305''$ *s* of θ Orionis, was, on 1878, April 3, extremely faint, being only of 12·8 mag., whilst on April 4, 5, and 19 it had decidedly increased in brightness, and was then of 9·7 mag. Dr. Schmidt also calls attention to the star B. D. $19^h\ 45^m\ 55^s{\cdot}7\ +24^\circ\ 37'{\cdot}0$, which Heis notes as of 5 and 5·6 mag. in his 'Uranometria' and Catalogue respectively, but which, in September last, was of 6·7 or 6·8 mag. Its magnitude is given by Lalande as 5, by Bessel as 7, and by Argelander as 5·7.

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[*] Comptes Rendus, Vol. lxxxvi. No. 25.

[†] Astron. Nachr. Nos. 2213, 2220, 2221, 2227.

Herr Schwab finds evidence of variability of both γ and ε Pegasi. The former has a period of 27½ days, and it is equal in brightness to α Pegasi at maximum and to η Pegasi at minimum. We are inclined to think, however, that α and η Pegasi are also themselves variable. The period of ε Pegasi appears to be about 25¾ days. Herr Schwab satisfied himself that the apparent variability was not caused by the presence of the Moon, although it was near the star at the time of minimum.

M. Palisa finds from his observations a period of 220 days for the new variable V Scorpii: the last maximum occurred in the middle of March last. Herr Dunér announces the discovery of a new variable, 182 Schj., which he proposes to designate V Coronæ. The star is marked as of 8·5 mag. in the Bonn Durchmusterung; but, on April 21 last, he found it was barely of 10·5 mag. On examining it again on Sept. 20, it was of 7·8 mag., and by Oct. 5 had risen to 7·5 mag. Herschel, in his Cape Observations, gives its magnitude as 9·5, and Birmingham on 1873, March 14 and 19 and April 20, and 1874, April 10, was not able to see the star at all; and Ball, on 1876, Aug. 1, could find no star of 8·5 mag. in the place.

A NEW BINARY*.—Mr. Burnham finds that the distance of the small companion of 99 Herculis, which was about 1″·7 in 1859, when Mr. Alvan Clark discovered it at Mr. Dawes's observatory, has now decreased to 1″·0, so that, on account of the faintness of the companion (10½ mag.), this is now a moderately difficult object with the 18½-inch refractor of the Dearborn Observatory, Chicago. The position-angle has in the interval increased from 347°·2 in 1859 to 24°·4 in 1878, or at the rate of about 2° per annum.

STAR-SPECTRA †.—Herr Dunér gives a list of stars of Secchi's types III. and IV., which he has spectroscopically examined at Lund, with descriptions of peculiarities in their spectra. Twenty-five stars belong to type III.=class III. *a* of Vogel, and fifteen to type IV. or class III. *b*, the latter being mostly faint stars. In type III. are included the variables R Ursæ Majoris, R Aquilæ, S Coronæ, T Ursæ Majoris, R Aquarii, R Cancri, R Vulpeculæ, T Cassiopeæ, and also μ Ursæ Majoris, λ Draconis, and λ Ursæ Minoris; in type IV., V Coronæ, S Cephei, and a number of red stars on Schjellerup's and Birmingham's lists. Herr Dunér makes a few corrections in Secchi's 'Catalogue of Star-Spectra.'

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* Astron. Nachr. No. 2214.

† Astron. Nachr. Nos. 2209, 2228.

NEW METHOD FOR DETERMINING ASTRONOMICAL FLEXURE*. —Several plans have been proposed for determining this important instrumental correction, which is one of the chief causes of systematic error in catalogues of fundamental stars; but they appear to be all liable to objection, and no attempt has ever been made to apply any one of them in practice. M. Loewy has devised a new method which it is intended to apply to the Bischoffsheim meridian-circle. He proposes to place at the centre of the central cube of the instrument a concavo-convex lens from 1½ to 3 inches diameter and about 1 inch thick, having its concavity turned towards the eyepiece, and of radius equal to its distance from the wire frame, so that a reflected image of the wire would be formed by its side, and thus the flexure of the eye-end would be determined as the telescope is turned. The convex surface of the lens is worked to such a radius that its focal length is one-fourth that of the object-glass; and thus the image of a point on the object-glass is also formed by the side of the wire, and being observed in the eyepiece as the telescope is turned, the sum of the flexures of the two ends of the instrument is determined. In both cases the result is affected by any small displacement of the lens in the cube; but this M. Loewy proposes to determine and allow for in the following manner. At the two sides of the lens towards the pivots of the transit-circle are worked two surfaces, one spherical, the other a plane inclined 45° to the axis. The rays from a point in one of the pivots, after falling on the convex surface, are reflected by the plane to the eye-end, where they form an image of the point, the curvature of the convex surface being such as to bring them to a focus. By comparing the images of the wire and of the point on the object-glass with that from the pivot, it would be easy, by combining the results, to eliminate the effect of the apparatus. Experiments have been made as to the effect of spherical and chromatic aberration with such a lens; and M. Loewy finds that with an aperture of from 0·6 to 1·2 inch the images are slightly coloured and perfectly sharp. Admiral Mouchez has sanctioned the construction of the apparatus, and MM. Henry have undertaken the task.

A FINE PROMINENCE was seen by Mr. John Hickley at Walton, near Glastonbury, on 1878, Dec. 20, from 0^h to 1^h. The chromosphere in this part was very bright and ragged, and from it rose, of uniform brightness and sharpness, a prominence which went through various changes till it almost or quite disappeared. First, it was a pointed wave or cone, then rose higher and the greater part became detached from the base, and, after fluctuations, became gradually broken up into pieces which rose to a great height and covered a great space. There were no violent movements, but a constant and rapid change. This was seen with the slit tangential. When placed radial, near the end of the display, the dispersed portions were seen extending a long way upon the Fraunhofer line C. Mr. Hickley also saw a large prominence on 1879, Jan. 2, but this underwent no change. He remarks that this prominence was less dense and bright than the chromosphere, whereas the uplifted portion of that seen on Dec. 20 was of the same shade and apparent density as the chromosphere; and when this is the case he has learnt that change is likely to occur.

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* Comptes Rendus, Vol. lxxxvii. No. 24.

CHANGES IN THE LATITUDE OF THE PARIS OBSERVATORY*.—The question of the stability of the vertical having engaged the attention of several astronomers, and in particular of M. d'Abbadie (*cf.* 'Observatory, No. 14), Admiral Mouchez, the Director of the Paris Observatory, has taken up the subject, and proposes to devote the Bischoffsheim meridian-circle by Eichens to the investigation. At his suggestion, M. Gaillot has discussed all available determinations of latitude with the Gambey circle, of which there are 1077 made by 10 observers in the period 1856–1861. The changes are examined under four heads:—(1) Progressive, (2) Annual, (3) Diurnal, (4) Accidental. (1) Progressive changes: The differences in the results for the several years are evidently accidental, being alternately positive and negative, and of the same order as the differences between the results of the several observers. The extreme range in the yearly means for latitude is 0″·63. (2) Annual changes: The discussion of these was undertaken in 1865 at M. Le Verrier's request; and the following are the results for the excess of each month over the mean of the year:—

Jan.	Feb.	Mar.	Apr.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
″	″	″	″	″	″	″	″	″	″	″	″
−0·23	−0·06	−0·03	+0·10	+0·16	+0·25	+0·25	+0·16	+0·13	−0·07	−0·11	−0·27

The change though small is regular and well marked, and may be represented by the formula

$$\delta\lambda = +0''\cdot20 \sin\left[\frac{360^\circ}{365\frac{1}{4}} \times (t-95)\right],$$

where t is the number of days elapsed since Jan. 1. Without excluding the possibility of an annual oscillation of the vertical, M. Gaillot thinks that the variation may be explained by small changes, dependent on temperature, in the coefficients of refraction and flexure. (3) Diurnal changes: The day-observations in 1856 give 11″·78, and the night observations 11″·81 for the seconds of latitude, thus showing no trace of a diurnal variation. (4) Accidental changes: The extreme deviations from the mean are −3″·17 and +2″·48; and M. Gaillot considers that these fall within the limits of reasonable error of observations made in the midst of the bustle of a large city, which causes troublesome tremors in the mercury. He concludes, therefore, that there is no need to call in the aid of any deviations of the vertical to explain them, and that such deviations, if they exist, are too small to be determined by observation. Taking into account the uncertainty resulting from abnormal refraction, and from the imperfection of the means of observation, M. Gaillot estimates that the possible error of latitude could hardly exceed 0″·20.

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* Comptes Rendus, Vol. lxxxvii. No. 19.

VISIBILITY OF THE CORONA IN FULL SUNLIGHT*.—Prof. Tacchini records a remarkable view which he had of the corona on 1879 July 29, under very favourable atmospheric conditions at Palermo, the sky being of that exceptional transparency which is a sure indication of either a low or an upper current of the Sirocco. He hid the disk of the Sun, as he is in the habit of doing on similar occasions, and saw the corona so distinctly, that he made a sketch of it, which agrees remarkably with a drawing made on the same day in America by P. Sestini during the eclipse. At Palermo the corona was of course seen without the intervention of an eclipse, and the rays and streamers appear to have been distinctly traced to a distance of 1¼° from the Sun's centre on the one side, and of ¾° on the other. This observation is, as regards the extent to which the corona was visible, unique—though Mr. Brett many years ago pointed out that the corona could be seen in the neighbourhood of the Sun without an eclipse (a statement much ridiculed at the time), and several observers, including M. Janssen and Prof. Langley, have seen it as a bright background on which Venus and Mercury stood out as black disks outside the Sun's limb at the late transits.

In comparing his observations of the chromosphere on the day of the late eclipse with the representations of the corona, Prof. Tacchini finds that the most vivid rays in the latter correspond to the regions of iron and magnesium and 1474 K in the northern hemisphere; and thus, with the present minimum of spots and other solar phenomena, it would have been natural to expect an absence of well-defined vivid plumes, leaving merely a modest aureola with rays more marked in the northern hemisphere, as, in fact, was the case. He contrasts the photograph taken by Davis in India in 1871, showing 27 bright rays nearly all equal to the Moon's radius in extent, besides others smaller but very bright round the whole limb, with Draper's photograph in 1878, in which the absence of detail proves that the Sun was enveloped, as it were, in a nebulous stratum of uniform extent and brightness, just as the chromosphere and photosphere also are now uniformly calm. Notwithstanding certain discordances, the drawings and photographs all agree in indicating a great extension of the corona in the plane of the Sun's equator.

Prof. Tacchini considers that photography is incapable of reproducing the *ensemble* of the phenomena during an eclipse, just as it cannot reproduce the granulations; but he appears to have come to this conclusion without sufficient examination of the subject. In M. Janssen's photographs, and also in those taken daily at Greenwich, granules of a fraction of a second of arc in diameter are distinctly represented with one and the same exposure, not only in the middle of the disk, but also near the limb; and it is a question whether they can be seen so well with a telescope. It is undoubtedly true that a single photograph cannot well represent the dazzling prominences and the fainter portions of the corona; but by taking several with different exposures, the whole of the phenomena may be *faithfully* reproduced, which is more than can be said in the case of drawings.

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* Memorie degli Spettrosc. Ital. 1878, Oct.

New Nebulæ*.—M. Stephan, Director of the Marseilles Observatory, gives a list of 39 nebulæ discovered by him in 1878. Of these, two are in Sir J. Herschel's Catalogue (though there is an error in the N.P.D.); the others are presumably new. They are all faint objects. M. Stephan has already discovered a large number of new nebulæ, having been much favoured in his search by the pure sky of Marseilles.

Search for Vulcan.—At the Royal Observatory, Greenwich, a close scrutiny of the Sun's disk was kept up from sunrise on the morning of March 19 (civil reckoning) till past noon, and sixteen photographs were taken during the interval; but no spot of any sort was seen. We may therefore conclude that M. Oppolzer's hypothetical planet does not exist.

Senor Arcimis, writing from Cadiz, states that bad weather prevented him from keeping a regular look out; but from March 18, $22^h\ 45^m$, to March 19, $0^h\ 17^m$, he observed the Sun through breaks in the clouds, and saw nothing of the suspected planet.

In connexion with this question, P. Ferrari† has brought to light an interesting observation by the well-known astronomer De Vico. In the 'Memorie dell' Osservatorio del Collegio Romano' for 1838, De Vico, in speaking of his observations of sun-spots, adds that he watched carefully for transits of asteroids such as he had the fortune to observe in 1837. Decuppis, a friend of De Vico's, supplies the date of this observation in the 'Album' for 1838, July 7, where he states that on July 12, 1837, De Vico saw a very small and perfectly round spot, without trace of penumbra, traverse a good part of the Sun's disk in the short space of 6 hours.

Discovery of Minor Planets.—A new planet, of the eleventh magnitude (No. 192), was picked up by M. Palisa at Pola on Feb. 17, and another of the twelfth magnitnde (No. 193) by M. Coggia at Marseilles on Feb. 28.

Prof. Peters, at Clinton, N.Y., announces the discovery (presumably on March 21) of two minor planets of the eleventh and tenth magnitudes respectively.

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* Comptes Rendus, Vol. lxxxvii. No. 23.

† Comptes Rendus, Vol. lxxxviii. No. 9.

The Lalande (Astronomical) Prize of the French Academy has been awarded to M. Stanislaus Meunier for his researches on the constitution of meteorites, which, in the opinion of the Academy, justify him in the conclusion that these bodies formerly belonged to a globe of considerable size which, like our Earth, has had its successive geological periods and has afterwards broken up into fragments. The Valz Prize has been awarded to Dr. Julius Schmidt for his Map of the Moon. Editor.

Ephemeris of the two Outer Satellites of Uranus.

	Titania.		Oberon.	
8h G. M. T.	Position.	Distance.	Position.	Distance.
1879.	°	″	°	″
Apr. 16	281·2	9·0	16·8	43·9
17	208·9	23·6	9·5	44·9
18	194·2	33·8	1·0	37·0
19	182·1	28·5	344·0	22·8
20	146·2	12·1	280·2	12·1
21	39·9	17·4	219·6	23·4
22	18·3	31·7	203·3	37·4
23	6·9	32·0	194·8	44·8
24	346·6	18·2	187·5	43·3
25	242·8	11·4	177·8	33·5
26	203·4	27·8	154·6	18·6
27	190·9	33·7	75·4	13·4
28	176·6	24·2	33·6	27·4
29	110·5	9·1	20·7	39·9
30	30·5	22·4	12·9	45·0
May 1	14·9	33·2	5·3	41·2
10	11·6	33·5	119·4	12·5
11	357·9	24·9	45·8	20·1
12	299·3	9·3	25·5	34·4
13	212·1	21·2	16·3	43·2
14	195·6	32·6	9·0	43·5
15	183·7	29·3	0·1	35·3

A. M.

Ephemeris of Brorsen's Comet (by Prof. Schulze).

Berlin Noon.	R.A. h	m	s	Dec. °	′		Log *r*.	Log Δ.
May 8	7	21	45	64	56·7	N	9·9742	9·8350
10		55	44	65	26·3		9·9863	9·8346
12	8	30	12	65	26·5		9·9982	9·8355
14	9	3	49	64	57·9		0·0097	9·8375
16		35	22	64	3·0		0·0210	9·8407
18	10	4	6	62	45·7		0·0320	9·8450
20		29	44	61	10·3		0·0428	9·8503
22		52	16	59	21·3		0·0533	9·8567

Berlin Noon.	R.A. h m s	Dec. ° ′	Log r.	Log Δ.
May 24	11 11 58	57 22·4 N	0·0635	9·8640
26	29 10	55 17·1	0·0734	9·8721
28	44 13	53 7·9	0·0831	9·8811
30	11 57 27	50 57·1 N	0·0925	9·8907

The nearest approach to the Earth is on May 10, when the comet's distance from us is 0·68, or about 63 millions of miles.

The comet was observed by Herr Strasser at Kremsmünster on March 14, and he finds the correction to the ephemeris to be -31^s in R.A and $-3'{\cdot}5$ in Dec. He describes the comet as bright, with a nucleus in the middle, and easy to observe. Mr. Tempel states that on March 10 the comet was brighter than an 8-mag. star.

Astronomical Memoranda, 1879, *May.*

Sun. May 1, sets $7^h\ 20^m$, rises $16^h\ 32^m$; May 31, sets $8^h\ 3^m$, rises $15^h\ 51^m$.

Equation of time:—Sun *before* clock, May 1, $2^m\ 59^s$; May 31, $2^m\ 37^s$.

Sidereal Time at Mean Noon:—May 1, $2^h\ 36^m{\cdot}2$; May 31, $4^h\ 34^m{\cdot}5$.

Moon. Full, May 5, $18^h\ 12^m$; Last Quarter, May 12, $14^h\ 36^m$; New, May 20, $17^h\ 50^m$; First Quarter, May 28, $11^h\ 37^m$.

Occultations:—May 3, $9^h\ 19^m$ *q* Virginis Oc. D. 114°; May 9, $15^h\ 42^m$ B. A. C. 6490 Oc. R. 262°; May 26, $9^h\ 26^m$ 54 Cancri Oc. D. 12°. The angles at disappearance (D.) and reappearance (R.) are reckoned from the *apparent* N. point towards the right hand round the Moon's circumference as seen in an inverting telescope.

Mercury is a morning star, at greatest elongation W. (25° 40′) on May 14, when he rises 34^m before the Sun in the E.N.E.

Venus is an evening star, setting at $10^h\ 41^m$ on May 1 and at $11^h\ 16^m$ on May 31. Her disk is gibbous, the illuminated portion being 0·822 on May 1 and 0·696 on May 31, and the corresponding diameters $13''{\cdot}2$ and $15''{\cdot}8$.

Mars is a morning star in the S.E., advancing through Aquarius. He is in conjunction with Jupiter and 53′ S. of him on May 9.
May 1, R.A. $22^h\ 18^m{\cdot}9$, Dec. 12° 14′ S., tr. $19^h\ 42^m$, rises $14^h\ 39^m$.
May 31, 23 41 ·0, 4 17 S., 19 6 , 13 23 .

Jupiter is a morning star in the S.E., advancing through Aquarius.
May 1, R.A. $22^h\ 36^m{\cdot}8$, Dec. 9° 44′ S., tr. $19^h\ 58^m$, rises $14^h\ 44^m$.
May 31, 22 51 ·9, 8 21 S., 18 15 , 13 54

Jupiter's Satellites. May 18, $14^h\ 52^m$, ii. Tr. E.; May 19, $15^h\ 3^m$, iii. Oc. D.; May 20, $14^h\ 52^m$, i. Tr. I.; May 25, $14^h\ 38^m$, ii. Tr. I., $14^h\ 48^m$, ii. Sh. E.

Saturn is a morning star in Pisces, rising shortly before the Sun.

Uranus is an evening star, in quadrature on May 21, and nearly stationary throughout the month, 8^m *f.* and 24′ *s.* of Regulus, about $\frac{1}{3}$ of the way towards *ρ* Leonis. EDITOR.

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EDITED BY

W. H. M. CHRISTIE, M.A.

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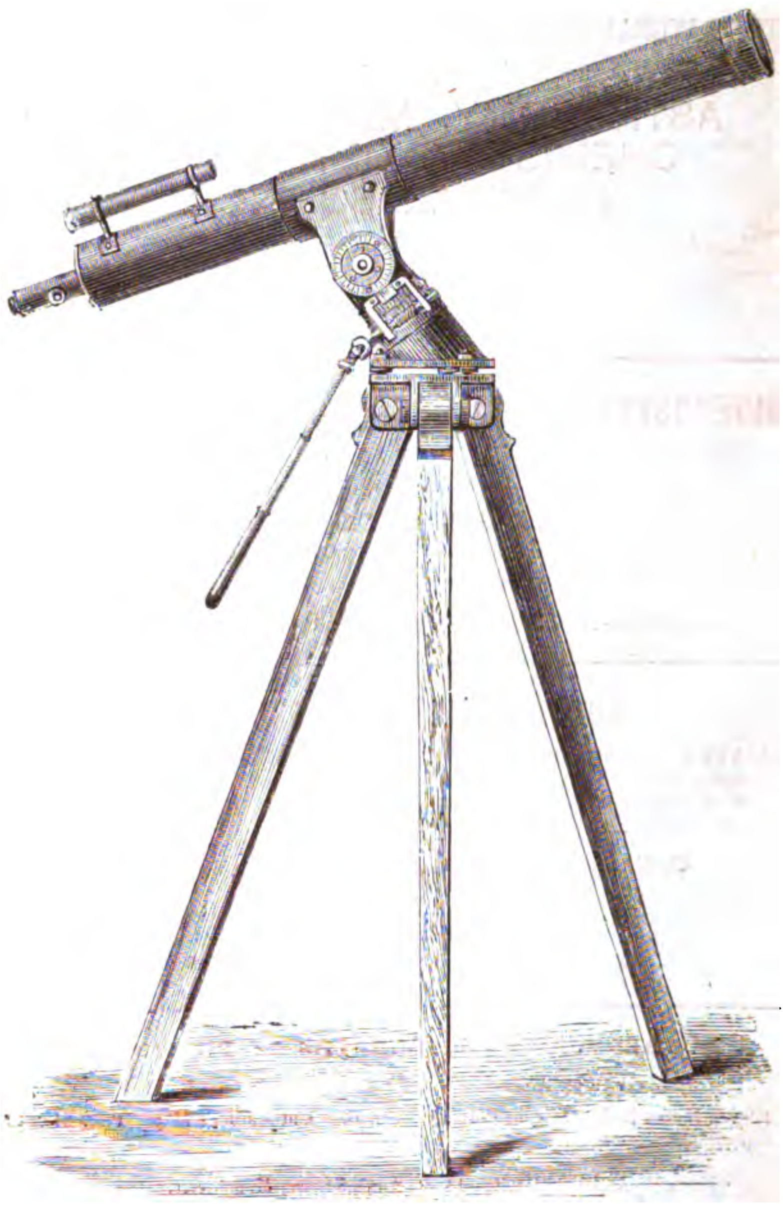

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